V. B. Lee

OBSERVATIONS

ON THE

NATURE AND CURE OF

CALCULUS, SEA SCURVY, CONSUMPTION, CATARRH, AND FEVER:

TOGETHER WITH

CONJECTURES UPON SEVERAL OTHER SUBJECTS OF

PHYSIOLOGY AND PATHOLOGY.

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TO THE

DISCOVERER

OF THE VIRTUES OF

VEGETABLE ALKALI, SUPERSATURATED WITH CARBONIC ACID,

SIR,

IT has been frequently with great confidence affirmed, that our acute pains are of ſhort duration. A very ſlight acquaintance, however, with the tremendous catalogue of human maladies, will ſatisfy us that this is the vain aphoriſm of a ſophiſt, more anxious to place words in oppoſition, than to obſerve the courſe of nature. Our excruciating diſeaſes are, if I do not compute very much amiſs, remarkable for length of paroxyſms, and for frequency of recurrence; while in thoſe of a different character, languor and depreſſion are ſcarce leſs intolerable than the moſt intenſe pain.

I hope,

I hope, and I believe, that this mighty maſs of evil will be gradually diminiſhed, and finally diſappear from the face of the earth. We are juſt beginning to catch a glimpſe of the laws of animal nature; and now, when the human mind ſeems, in ſo many countries, about to be rouſed from that torpor, by which it has been ſo long benumbed, we may reaſonably indulge the expectation of a rapid progreſs in this, the moſt beneficial of all the ſciences. An infinitely ſmall portion of genius has hitherto been exerted in attempts to diminiſh the ſum of our painful ſenſations; and the force of ſociety has been excluſively at the diſpoſal of Deſpots and Juntos, the great artificers of human evil. Should an entire change in theſe two reſpects, any where take place, every member of ſociety might ſoon expect to experience, in his own perſon, the conſequence of ſo happy an innovation; and ſhould the example be generally followed, there is no improvement in the condition of the World, for which we might not hope from the bloodleſs rivalſhip of nations.

From Chemiſtry, which is daily unfolding the profoundeſt ſecrets of nature, and, among the reſt, the delicate play of living machinery, your example alone would juſtify us in entertaining the moſt ſanguine expectations: ſince the earlieſt diſcoveries in that department of chemiſtry, which has been ſo ſucceſsfully cultivated by BLACK, CAVENDISH, PRIESTLEY, SCHEELE, and LAVOISIER, ſuggeſted to you a ſafe and efficacious remedy for one of the moſt frequent, painful, and hopeleſs of diſeaſes.

Much as you have contributed, by the frank and diſintereſted communication of your diſcovery, to obliterate one of the darkeſt ſhades from the proſpect of life, your name is, I ſuſpect, ſcarce known beyond the narrow circle of the practitioners of medicine, except, perhaps, to a few among thoſe who are indebted to you for eaſe and health. Such is the inattention of mankind to their beſt benefactors! and ſo entirely have fatal illuſions preverted our moral ſentiments! I cannot hope to add much to your

reputation;

reputation; but by attempting to diffufe more widely the benefit for which mankind are originally indebted to you, I may perhaps afford you gratification.

That the former part of the following pamphlet will do fome good, I am confident; though I do not believe that alkaline medicines will relieve calculous diforders under every form. Thofe diforders, befide the different feats they occupy in different perfons, appear alfo, from the analyfis of various calculi, to be liable to confiderable variation in their nature.

The fpeculations that follow, will, perhaps, appear to you too remote from application, and my hopes of the future improvement of medicine too high-flying.

It is, I am fenfible, but a poor expedient, to lay one's felf out for the praife of ingenuity by propofing projects which are in no danger of being difgraced by trial; nor have I ever much regarded medical obfervations, that are of no other uſe than to be read. But this, I can aſſure you ſhall not be the caſe here; and the more I reflect, the more confident I become, that an eaſy and convenient method of offering phthiſical patients a chance of recovery, which has never yet, upon any probable grounds been offered them, will ſhortly be contrived. For *typhus*, if the light that is now dawning upon phyſiology and pathology does not preſent objects to me under very illuſive forms, we ſhall not fail to ſtrike out an almoſt infallible method of cure; and this method, I think it probable, will extend to the ſcarlet fever alſo; which is perhaps the moſt formidable among the acute diſeaſes of this climate. In the treament of fevers we have, it is true, learned to avoid ſome fatal miſtakes of our anceſtors; but we can boaſt of little elſe. In thoſe caſes in which alone there is, perhaps, occaſion for the interference of art, art ſeems almoſt impotent: from attention to the ſingle circumſtance of debility, I imagine, that patients are often drenched with wine and opiates, till they are ſtimulated to death. If I have imputed

imputed the debility to its real cauſe, our chief aim ſhould be to reſtore the principle of excitability; and ſtimulants ſhould in the mean time be adminiſtered with a more ſparing hand. Perhaps, when the proper method of reſtoring this principle ſhall have been deviſed, *extraordinary* ſtimulants will become unneceſſary. The Materia Medica was once ſuppoſed to contain diſtinct ſpecifics for the diſeaſes of each ſeparate organ; it is now regarded as little elſe than a collection of ſtimuli; ſo that medicine is become the art of adminiſtering drams. Hence it can often only amuſe or palliate, and muſt ſometimes injure, by forcing into motion, conſtitutions already too much worn. How would our reſources be multiplied, if we could give excitability or life, as well as ſtimulants! "But "is ſo ſalutary a revolution in medicine "poſſible?" I do not know; but is it not worth while to enquire?

I am, SIR,

Reſpectfully your's,

THOMAS BEDDOES.

Oxford,
30*th July*, 1792.

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AN

ACCOUNT

OF A

SIMPLER METHOD OF TREATING

Certain Calculous Complaints.

BOOKS and tradition never fail to offer a multitude of medicines for diſeaſes that are frequent and incurable; many of theſe medicines are the ſuggeſtion of the moſt fantaſtic analogies, and the greater part are incapable of even palliating for a moment the ſufferings of the patient; yet a liſt, at firſt ſight ſo unpromiſing, is not abſolutely without its uſe. The phyſician ſtalks about with an air of greater dignity when he feels a full quiver at his ſhoulders, however blunt may be the arrows it contains; and it ſupplies a ſtaff,

 however,

however feeble, on which the wearied ſpirits of the patient may reſt, and defers a little that ſeaſon of ſettled gloom when Futurity has nothing to promiſe to Hope.

There exiſts, probably, no human malady, not even the jaundice, conſumption, ague, cancer, or dropſy excepted, for which ſo many whimſical and nugatory means of relief have been propoſed, as for the ſtone and gravel. Beſides an infinity of inefficacious ſimples, the whole ſeries of remedies, from the warm goat's blood of Alexander Trallianus, the pounded glaſs of Baricellus a Sancto Marco, § the eſſence of pigeon's dung of Johannes Poppius, the *quinta eſſentia urinæ humanæ* of Fabri, the *ſpiritus microcoſmi e ſtercore humano* of another chemical or alchemical doctor,

§ *Incredibile nec forté unquam tentatum remedium*, ſays Dr. Siebold. Yet the powder occaſionally found within the cavity of ſtones is ſtill uſed as a remedy for calculous complaints in our chalk counties; I met with an inſtance of this kind within theſe few days. It was probably conceived, in both caſes, that one kind of grit would draw or drive the other out of the body.

doctor, down to the bearberry of De Haen, at once afford a proof of the inefficacy of each particular medicine, and of the prevalence of a diforder which could enforce fo much attention, and fuggeft fo many extravagant projects.

From the teftimonies that have fallen under my obfervation, I can collect, that during the former part of the prefent century fome approaches had been making towards a remedy, which, whatever may be its mode of operation, or precife degree of efficacy, is undoubtedly capable both of relieving that pain, which renders the diforder fo formidable, and of fufpending the progrefs of the difeafe itfelf. As early as the year 1721, Robinfon propofed falt of tartar, among other things, as a folvent for the ftone *. In diforders of the urinary organs, whether arifing from concretions or not, Hoffman praifes the efficacy of the hot alkaline fprings of Germany, as

* Treatife of the Stone and Gravel, 1721.

well as of the ſalt obtained from the waters of Carlſbad. At a later period, alkaline, ſubſtances enriched the Engliſh empirics, and obtained the commendation of Hartley, Whytt, Kirkpatrick, De Haen, and other phyſicians of great celebrity. It appeared, indeed, that the concretions were not really diſſolved, even in the more favourable caſes; yet the pain was permanently relieved. Every phyſician is acquainted with the very curious facts on this ſubject related by Whytt, and more eſpecially by De Haen *. Yet, notwithſtanding ſome partial ſucceſs, theſe cauſtic materials were afterwards generally

* See thee firſt volumes of De Haen's Ratio Medendi. The caſe of that patient who had ſwallowed eight hundred quarts of lime-water in half a year, and who continued free from pain for three years, though he had ſtill a ſtone in his bladder, is very remarkable. The ninth caſe, related in the "*Account of the Efficacy*," &c. affords another ſuch inſtance of palliation. The patient appears to have been kept more than tolerably eaſy by the ſuperſaturated ſolution for two years and an half; yet, from the concluding expreſſion of his letter, "*I am ſeldom troubled with any pain*," I infer that the diſeaſe ſtill exiſted in the bladder.

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laid aſide. They were, however, laid aſide with regret, ſince this inference ſeemed to be warranted by the whole ſum of facts that much benefit might be derived from them, provided their collateral bad effects could be obviated. This concluſion appears evidently to have reſted upon the mind of Profeſſor Bergman, who, as far as this diſorder is concerned, may be quoted as an authority in medicine: in a paper publiſhed in 1776 he has theſe words—" *hoc unum addo, calculi analyſin chemicam medicæ arti haud parum utilitatis polliceri, calcis vivæ aquam et lixivium cauſticum alkalinum calculo mederi experientiâ conſtitit. Idem vero ſi ignotum hactenus fuiſſet ex ipſâ calculi mixtione nunc inventâ et detectâ intelligeretur.*

It was reſerved for a reſpectable member of the medical profeſſion, ſtill living, to engage the modern chemiſtry in the ſervice of medicine, and realize a project which now ſeemed to be relinquiſhed in deſpair. This gentleman's reflections were quickened by his own feelings, and in

1778,

1778, after having been for eighteen years ſubject to ſevere nephritic paroxyſms, he began to take a ſolution of fixed vegetable alkali, ſuperſaturated with carbonic acid, or fixed air. This medicine ver7 ſoon relieved his calculous ſymptoms, and, as it appears from the account of his caſe, has kept him free from pain for ten years, one ſlight attack excepted, which is aſcribed to the diſcontinuance of the medicine for ſeveral weeks. Perhaps it might be ſerviceable to mankind, if medical practitioners, attentive to the progreſs of ſcience, and capable of combining ideas, were from time to time to be ſeized by thoſe diſeaſes for whic remed ies are ſtill wanting. The narrative of this caſe, with that of upwards of twenty others, is contained in a pamphlet, now well known under the title of "*An account of the Efficacy of Aqua Mephitica Alkalina, &c. in Calculous Diſorders.*" Experience has ſince amply confirmed the virtues of a medicine, which, I apprehend, may be feely taken without danger, and even without inconvenience, except in a few

rare

rare inſtances, and which ſeems to have deſerved the ſingular praiſe of equalling the expectations raiſed by the perſon who firſt propoſed it. The method of preparing this medicine is as follows: Diſſolve two ounces and a half (troy weight) of dry ſalt of tartar in five quarts (wine meaſure) of ſoft water; after ſtirring the water, and then ſuffering it to ſtand long enough for the ſubſtances generally precipitated from water by fixed alkali, and the reſiduum of the ſalt of tartar itſelf to ſubſide, pour off the clear ſolution, and place it in the middle veſſel of Parker's apparatus for impregnating liquids with fixed air, and expoſe it for forty-eight hours to a ſtream of that elaſtic fluid. Of the liquor, from twelve to twenty-four ounces have been taken every day by different perſons afflicted with various calculous complaints, and always, except in one inſtance, with the deſired effect, after it has been continued ſome time.

One might perhaps, be diſpoſed to wiſh that this remedy could be prepared with

leſs

lefs trouble and attention ; but the great defideratum is a cheap, fafe, and efficacious formula, adapted to the poor, who are by no means exempted from calculous diforders ; for when the high price and brittlenefs of the apparatus is confidered, and when we likewife take into the account the neceffity of conftantly continuing the medicine, in order to prevent the return of the difeafe it will appear probable, that the poor are not often likely to reap the full benefit of the difcovery.

In the year 1786 or 1787, a perfon belonging to the medical profeffion, and much afflicted with the gravel, complained to me that he was unable to perfevere in the ufe of aqua mephitica alkalina, on account of the great dizzinefs it always occafioned *. I was

* Dizzinefs with a degree of intoxication, is a very common effect of liquors containing carbonic acid. Pyrmont water, which contains of carbonic acid air confiderably more than a quantity equal to the bulk of the water itfelf, occafions a glow, exhilaration, and con-

I was led by this intimation to reflect upon the fubject, and after fome time fell upon what indeed was abundantly obvious; a formula of which I think myfelf fully warranted in afferting, that it is extremely beneficial in calculous complaints, and that it may, without injury, be taken in very large quantities, and continued for a great length of time. Its fimplicity and its cheapnefs are its great recommendations. I cannot determine, for want of comparative ob-

fufion of ideas fimilar to that which follows the ufe or abufe of fpirituous liquors. This effect is fo common as to have given rife to a particular term, *brunnenraufch*. A fpring of the fame quality, which the Tartars, on this account, call *the well of drunkennefs*, is mentioned by one of the late travellers into Siberia. Some bottled liquors, I fuppofe, in part, owe their intoxicating power, and more efpecially the fuddennefs of their effect, to carbonic acid. This acid is produced in the bottles by a flow continuance of the vinous fermentation, and therefore thefe liquors will contain more alcohol when they are ripe, than at the time of bottling; but I do not imagine this difference will account fatisfactorily for the prodigious difference of their effect upon the head. Sparkling Champagne probably owes its exhilarating power to the fame caufe.

ſervations, whether it is inferior to the more operoſe preparation in efficacy ; and how much inferior ; there can be no reaſon to ſuppoſe that it is ſuperior : and were both equally eaſy to procure by all patients, I ſhould not think it worthy of public notice.

The formula I have employed for two or three years paſt is as follows : take natron or ſal ſodæ in cryſtals, pound it coarſely, and expoſe it to a warm dry air, till it entirely crumbles into a white powder ; make this powder into pills with ſoap or any other cement ; aromatics, extract of bark, &c. may be added ; but I have never found any addition neceſſary ; a quantity of ſoap, rather more than equal to the weight of the calcined alkali, is neceſſary to make it into pills. *

In

* I now ſee that Dr. Donald Monro (*Materia Medica*, article Alkali, 1788), had an idea of giving foſſil alkali in pills, in calculous caſes, " It may," ſays he, and this is all he ſays, " be made into pills or boluſſes,

" mixed

In order to expedite the expulſion of the water of cryſtallization, the alkali may be ſpread out before a fire, or the veſſel containing it may be placed in boiling water, and the pounded cryſtals ſtirred till they have loſt rather more than half their weight: the recent cryſtals contain indeed ſixty-four parts of water in an hundred; but unleſs kept cloſe, they loſe part of this water, and it is prudent not to carry the calcination too far, if artificial heat be applied; nor ſhould the heat exceed that of boiling water, leſt any of the volatile acid ſhould be expelled; of this powder, from one to two ſcruples taken every day has generally af-

" mixed with ſome powder of liquorice root, by means " of gum arabic mucilage, or conſerve." From ſeveral articles of that work I had, perhaps haſtily, concluded that it fell ſhort both of modern chemical, and medical knowledge, and this paſſage had eſcaped me. I ſuppoſe the author, as he gives no intimation of the contrary, means to propoſe the cryſtallized alkali for pills. I have often found the pills apt to fall to pieces, when the water of cryſtallization is not ſufficiently expelled; I ſhould think this would happen ſtill more, when none is expelled.

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forded relief in leſs than three weeks; and in no caſe but one, out of more than twenty that have fallen under my own obſervation, have they failed to perform every thing which could be deſired from medicine, except eradicating the tendency to form calculous concretions, to which no known remedy has the ſmalleſt pretenſions.

I might perhaps ſafely truſt to the abovementioned pamphlet, as bearing abundant teſtimony to the efficacy of alkaline ſalts; and confidently appeal to future experience in confirmation both of the power of ſal ſodæ in the form preſcribed, and of its harmleſſneſs; at all events it will be unneceſſary to particularize ſlight caſes, which have always at once yielded to the remedy. The following, in which the ſymptoms were either of very long ſtanding, or extreme ſeverity, will, I hope, be ſufficient to procure a trial to the medicine.

I. Mr.

I.

Mr. William Rushton, of the Wyke, near Shifnal, Shropſhire, had been haraſſed for a conſiderable number of years by excruciating pains in his loins, attended with occaſional ſickneſs, and an almoſt total inability to ſtoop; the pains were accuſtomed to ſpread in all directions, and ſeverely to affect his head. He had at different times diſcharged much gravel; his urine formed depoſitions, was often extremely offenſive, and full of mucus; he was at times afraid to diſcharge it, ſo much were the paſſages irritated, and ſo intenſe the pain ſucceeding the evacuation. In July, 1787, he began to take a drachm of chryſtallized foſſil alkali, diſſolved in a quart of water, every day. In a few days he felt relieved, and in leſs than a month ſeemed, as he expreſſed it, *to be quite another man.* I have ſeen him repeatedly ſince; his ſufferings from his complaints have been very inconſiderable; but having ſometimes neglected the

the medicine for months together, he has felt ſome ſtiffneſs rather than pain acroſs his loins, which has immediately been removed by a repetition of the alkali.

The ſolution ſometimes produced a ſlight nauſea, againſt which his dread of the pains determined him to bear up. The pills have never been attended with the ſlighteſt unpleaſant ſenſation. During his long experience he obſerved, that ſtale (acid) beer never failed to bring on a ſevere paroxyſm. Several other perſons have repeated the ſame obſervation. On the preſumption ariſing from this information, I have always enjoined abſtinence from malt liquor in that ſtate; no other particular reſtriction of diet has appeared neceſſary, yet the inhabitants of cyder countries, as I have been informed upon inquiry, are remarkably free from this diſeaſe. Do the native and acetous acids differ in their effects? I ſhould imagine not. The ſpeedy effect of ſour beer ſeems to ſhew that it does not act by producing new concretions, but by ſome irritating power.

power. Would cyder affect a perſon ſubject to calculous paroxyſms in the ſame manner? Diuretics, as diluted ſpirits, generally do miſchief: the ſame remark, I think, occurs in the pamphlet quoted above. May, 1792, Mr. R. gave me the uſual favourable account of himſelf.

II.

BRAMAH, a workman in the foundery of Mr. Dearman, of Birmingham, was become quite emaciated, and unequal to his labour, from a gravelly complaint, under which he had laboured many years. He had long been accuſtomed to diſcharge concreted matter, and ſmall ſtones; his urine depoſited an incruſtation, and ſoon became fœtid; the pains at his loins were intenſe, &c. &c. He began to take the ſolution as before in October 1787, found relief in a few weeks, and ſoon conſidered himſelf as radically cured. Notwithſtanding my repeated admonitions, he has at times neglected to continue the medicine, and has had returns

of

of his ſymptoms. I never have ſeen an inſtance which more evidently ſhowed how ſoon we forget the moſt acute pain.

Having ſo frequently experienced the efficacy of the medicine, he now chooſes to ſuffer the diſeaſe to return in a ſlight degree, and then for two or three days to take a *handful* of pills; the conſequence of which is a diſcharge of gravel, after which he feels no farther inconvenience for months. I have not been able to diſſuade him from this violent method of treating himſelf; he has not, however, ſuffered from it.

My friend, Mr. I. Dearman, in a letter dated May 2, 1789, gave me the following account of one of his relapſes, for he has ſo often relapſed and recovered, that his caſe alone is equal to half a dozen, in proof of the power of the remedy: "He began "with half an ounce of ſal ſodæ, with as "much common ſoap as with the addition "of a little gum arabic made ſixty pills. "On ſecond day (Monday) he took four, 'increaſing the number every day, till 'yeſterday he took twelve, wihout the 'leaſt affection of the ſtomach or increaſe 'of urine; nor can he give any other 'account of their effect, than that the 'pain is removing very faſt, and he does 'not doubt but a few more pills will cure 'him. I have not been without an appre-'henſion of danger from his taking them ſo 'largely—he treats my remonſtrances very 'cavalierly, and ſays he ſhould not mind 'taking a box full of ſuch pills. He does 'not recollect any increaſe of urine when 'he was taking the medicine before.' Mr. Biddle, chemiſt, of Birmingham, in a letter dated June 1792, informs me, that 'Bramah 'and Wilks, at the foundery, have con-'ſtantly taken the pills, when they felt the 'complaint coming, and have as uniformly 'been relieved.'

III.

WILKS, a fellow-workman with Bramah, and not quite ſo terribly afflicted

 with

with nephritic ſymptoms, found perfect relief from the pills. After every other diſagreeable feeling was removed, he continued for ſome time to have a ſtarting of one of his thighs. Suſpecting this to ariſe from ſome cauſe of irritation lodged in the urinary paſſages, or at leaſt to have ſome connection with his calculous diſorder, I adviſed him to perſevere ſteadily in the uſe of the pills; and he has ſince informed me, that he is no longer diſturbed by this involuntary motion of his muſcles. He alſo informed me, that he had cured two or three other perſons with his ſpare pills, of which he ſeemed very deſirous to know the compoſition, having ſome idea, as I ſuſpected, upon the ſtrength of his experience of their virtues, of trying his fortune in the practice of medicine.

IV.

December, 1788. John Bucknall, labourer, of Kemberton, near Shifnall, Shropſhire,

ſhire, has had the gravel for thirty years, and very violently for the laſt ſix or ſeven years. In harveſt the pains at his loins have always been moſt ſervere, eſpecially when he was reaping or mowing, a kind of labour which his diſorder has often rendered him incapable of following. The harveſt beer is ſeldom without ſome degree of acidity, to which he imputes, in a great meaſure, the ſeverity of his ſufferings at that particular ſeaſon. The attitude of a mower or a reaper may, however, alſo account for this aggravation. He was directed to take a drachm of cryſtals of ſal ſodæ diſſolved in three half pints of water. I believe, however, he much exceeded this doſe, as the common people are ſo apt to do. In three days he perceived an increaſe of his urine, and a great deal of red gravel came away. He had now the ſenſation of a large lump in his right groin, which in about nine days totally diſappeared.

January 11, 1789. He conſidered himſelf as totally cured. He was ordered to take every day, for a month, a ſcruple of the ſalt diſſolved in water, at two doſes, and afterwards to repeat the medicine in the ſame doſe, one week at leaſt in every month; and if he found the quantity too ſmall to prevent the return of his diſorder, to regulate the treatment of himſelf according to his feelings, which a little experience would ſoon enable him to do.

I did not ſee him again till May, 1792, when he told me that he had long totally diſcontinued the ſolution, and that he had been perfectly free from any gravelly ſymptom till within a few weeks, when he perceived a ſtiffneſs in his loins, and ſome other indications of a return of his diſorder. I recommended to him the pills, but have heard nothing further concerning him. I met with one patient who would not exchange the ſolution for the pills. He is the only perſon I have ſeen, who thought the taſte of alkali not diſagreeable.

V. The

V.

The cafe of Mr. Roe, timber-merchant, at Newport, in Shropfhire, was remarkable, on account of the violence of the fymptoms, and the extreme irritability of the patient's habit. He complained of violent pains acrofs his loins, accompanied with ficknefs and vomiting, and intolerable fufferings immediately after the difcharge of his urine, and a conftant irritation of the bladder and urethra. He had from time to time difcharged much gravel, and many calculi as large as horfe beans. He was not able to ride without making bloody water. More than one of the former patients had mentioned this circumftance; but I believe the coffee-coloured is often miftaken for bloody urine.

Early in 1789 he took half a drachm of cryftallized foffil alkali, with three or four

four grains of cream of tartar * every day for three weeks. His pains were lessibly

* I then thought that the addition of a little cream of tartar to the alkaline folution, at the time of taking it, would gradually extricate fome fixed air in the ftomach and fecure the faturation. I even fuppofed that I might, by a few experiments, difcover fuch a proportion of thefe ingredient sa s would effect, without any apparatus, a fuperfaturation of the alkali that fhould remain uncombined with the acid of tartar. But the pills fuperfeded the neceffity of further refearches. On the addition of five or fix grains of cream of tartar to a fcruple, or a fcruple and a half, of fodæ, diffolved in twelve or fourteen ounces of water, clofing the mouth of the phial, and inverting it in water, the alkaline folution acquires a much lefs difagreeable tafte than a mere folution of equal ftrength has, at the moment of unclofing the phial, an effort of protrufion is felt, and a few air bubbles afcend. The neutral falt thus formed would itfelf be probably beneficial ; I cannot believe, at leaft, that the carbonic acid contributes, by any direct operation, to the cure or palliation of the difeafe, fince cauftic alkaline fubftances clearly feem, if they could be fafely taken, to have equal efficacy with the carbonated; and whatever be the effect of this acid, fome of thofe other acids, that are eafily diflodged from alkalis, would produce it equally. A refpectable obferver (Menghini, Comment. Bonon. v. 61, &c.) recommends Rochelle falt in calculous cafes.

ſibly diminiſhed, and the pills were now ſubſtituted in the place of the ſolution. In a letter dated March 23, 1789, he deſcribes himſelf as taking the pills: 'And 'tolerably well all the time, with now and 'then ſome little pain, after which a ſmall 'quantity of ſmall gravel comes away.' In a letter, dated 12th April, 1789, Mr Jones who attended him, ſays, 'There is no doubt 'as to Mr. Roe's caſe; I am very happy 'to inform you that he continues much 'better.' He took the pills regularly, felt from time to time ſome ſlight pains or intimations of pain, after which ſome gravel was diſcharged. In April, 1792, he called upon me to inform me that he had enjoyed eaſe and a good ſtate of health ſince I had heard from him laſt.

VI.

The follow ingletter contains an account of a rare irritability of the urinary organs. The medical reader will certainly adopt the writer's writer's opinion, that it is not a calculous caſe. I ought perhaps to have ſuppreſſed the concluſion of the letter; but as the fervour of expreſſion, doubtleſs, correſponds to the pain the writer was once accuſtomed to ſuffer, and the relief he afterwards experienced, it is properly to be conſidered as deſcriptive of the caſe, eſpecially as I have not the ſatisfaction of knowing the perſon who gives ſo much credit to the medicine:

'Sir,

'At the requeſt of my brother-in-law, 'Mr. Biddle, I take the liberty of informing you of the effect which the alkaline 'pills, recommended by you, have invariably produced upon me; but it is ne'ceſſary to premiſe, that my caſe appears 'to me of a very dubious nature, as both 'the quantity and quality of the ſubſtance 'which I ſometimes void by urine are in'ſufficient to convince me that my com'plaint is the gravel. But of that Sir, 'you may better form an opinion, when I 'mention, thata quantity of wine, by no 'means intemperate, generally produced ſo 'much pain in my kidneys, that to ſtoop, 'or in any manner bend my back, was 'impoſſible; nor could I eaſily turn myſelf 'in bed while this pain laſted, which 'moſtly laſted a week or two, though by 'drinking plentifully of diluting liquors I 'made water without difficulty; yet the 'laſt few drops, and ſometimes the quantity of a tea cup full, were very bloody, 'and came from me with excruciating 'pain; but I never diſcovered any gravel, 'and it appeared to me more like the mucus of the bladder: however, it more 'often happened that I had this fixed and 'intenſe pain in the ſmall of my back, 'without voiding any thing extraneous in 'my urine; and I am ſtill very liable, after 'the uſe of wine, to returns of it, but ſince 'Mr. Biddle has recommended it to me 'to try your pills, I have never failed, by 'uſing them for a day or two, or at fartheſt 'in the courſe of a week, to obtain effectual relief.

'These, Sir, are all the particulars of 'my case, which is not important enough 'to merit much attention, nor perhaps 'will it help much to establish the fossil 'alkali as a specific for the gravel, but to 'me it is a valuable discovery, and I have 'no doubt, when it is generally known, 'but it will be beneficial to thousands. I 'beg your pardon for the prolixity of this 'trifling detail, and earnestly desiring that 'you may obtain from others the praise 'and gratitude you have won from me, 'and that all your labours may be crowned 'with success, to the promotion of your 'own fame, and the happiness and im- 'provement of your fellow creatures, I 'beg leave, Sir, to subscribe myself,

'Your much obliged and

'very humble servant,

'*Lombard-street*,
'*June* 26, 1792.

'D. LLOYD.

Dr. BEDDOES,
Oxford.

The

The relation of the other facts that have fallen under my own notice would be ufelefs repetition. All the patients for whom I have directed the foffil alkali have been paft, and moft of them confiderably paft, the prime of life. Calculi are fometimes found in the bladders of children, but the nephritis calculofa feems to be one among the evils almoft peculiar to declining age. Yet the foffil alkali, whatever were the infirmities of thofe who took it, and in fome inftances they were very great, has always been perfectly harmlefs, and, but in one cafe, decifively beneficial in its effects.

The following communications will probably be confidered as ftrong additional proofs of its efficacy :

‘ DEAR SIR,

‘ I HAD the favour of your letter, and ‘ am happy to hear it is your intention to ‘ give the world your fentiments upon ‘ the efficacy of fal fodæ in calculous dif-

'eafes. I am fo well convinced of its 'good effects, that I look upon it as a 'valuable acquifition. As I write chiefly 'from memory, I fhall not be able, pro-'bably, to furnifh you with all the cafes 'that have fallen under my obfervation, 'but the few I have recollected you may 'depend upon the accuracy of. The pa-'tient, whom you enquire after, took it but 'a few days, loft his pain, and has fince 'had no return.

'Your thoughts upon fea fcurvy will 'be interefting to every one, but parti-'cularly fo to our failors and naval practi-'tioners. I have met with a few cafes (I 'think three) in this neighbourhood, but 'they were foon cured.

'I am your obedient fervant,

'J. JONES.

'*Newport*,
'25. *Feb.* 1791.

'CASE

' CASE I.

' JANUARY, 1789, N. N. had been for ' ſeveral years afflicted with gravel, had ' frequently violent fits of pain, and for ' ſome weeks paſt has ſcarcely ever been ' totally free; has with vaſt difficulty voided ' ſeveral large ſtones. He took fifteen ' grains of ſal ſoda, with one of magneſia, ' and cryſtals of tartar, twice a day for a ' fortnight; afterwards about ſeven grains ' of the dry powder of ſal ſoda, with as ' much ſoap formed into pills, for a fortnight ' or three weeks longer regularly. He ſoon ' diſcovered a large mucous and very ropy ' ſediment in his urine, and a great abate-' ment of pain; his fits returned at much ' longer intervals, were ſhorter, and much ' leſs violent, and the ſtones he voided ap-' peared half diſſolved, which before had ' been hard and rough, and when dry ' ſeemed to be covered with a fine ſoft ' powder. In ſix or ſeven weeks he be-' came ſo entirely free from his diſeaſe,

' that

‘ that he omitted his medicine for ſome ‘ months, and has ſince had recourſe to it ‘ occaſionally only for a fortnight at a ‘ time. He has never ſince had a ſevere ‘ fit, but now and then, perhaps once in ‘ three or four months ſome ſlight pain, ‘ and then voided ſoftened ſtones, more ‘ like pieces of hard clay than calculous ‘ concretions.

‘ CASE II.

‘ Mr. S. a farmer, who had lived ‘ freely, had been long tormented with the ‘ frequent paſſing of ſtones from the kid- ‘ neys. I was twice called to him, when ‘ they were ſo large as to ſtick firmly in ‘ the urethra, and were removed with con- ‘ ſiderable force by a pair of forceps, con- ‘ trived for the purpoſe, upon the principle ‘ of Smellie’s midwifery forceps.

‘ He had taken various medicines with- ‘ out any good effect, and upon that ac-

‘ count

'count was with difficulty prevailed upon 'to try the ſal ſodæ. In April, 1789, I 'give him the dry powder with ſoap (ʒij. 'of each in ſixty pills) of which he took 'two twice a day for near a month, from 'that time he has totally diſcontinued 'them, and when I laſt ſaw him (Oct. 1790) he had never had a return of his 'complaint that he thought worthy of 'attention.

'CASE III.

'In January, 1790, I was called to a 'farmer at ſome diſtance from me, who 'had for ſome years ſuffered much from 'a ſtone in the bladder. He informed me 'that he had taken a variety of what are 'called ſolvents, without the leaſt advan-'tage. He had now been for ſome time 'confined to his room, and had ſcarce 'any reſpite from pain. Under ſuch cir-'cumſtances I ſaw no proſpect of relief, 'but from the operation, which I ſtrongly

'recommended

'recommended to him. He could not 'be prevailed upon to ſubmit to it, 'but was very deſirous of trying any 'medicine I ſhould propoſe. The extra-'ordinary efficacy of the ... I had 'ſeen in the laſt caſe, ... me to try 'its ſolvent powers in this.

'He began upon it immediately, and 'took the uſual doſe twice a day ... 'months regularly. In the ſpace of ... 'days his pain abated; in a month he ... 'able to walk about his farm; and in the 'courſe of the ſummer walked ſeveral 'times ten or a dozen miles in the day. 'He ſtill continued the pills, but with leſs 'regularity, ſometimes omitting them for 'three or four weeeks; and when I laſt ſaw 'him, which was in October, remained more 'comfortable than he had been for the laſt 'two or three years.

' CASE IV.

' B. a neighbouring farmer, about 75, ' has been for ſome years tormented with a ' ſtone in the bladder. He had now (March, ' 1790) been for ſome weeks unable to leave ' the houſe, and in frequent and violent pain. ' He took the ſal ſodæ pills; in one week ' his pain was much relieved, and in a ſhort ' time he was able to go about as uſual. He ' ſtill continues much better, and able to ' ride gently, but is ſeldon many days with- ' out ſome pain. He ſoon became tired of ' the pills, and in the whole did not take ' them more than a month.'

Dr. Beddoes, *Oxford.*

In a letter dated June 16, 1792, Mr. Jones informs me, that of theſe patients the third has ſince died of another diſeaſe, and of the fourth, the ſymptoms have ſeveral times recurred, and that with far leſs ſeverity than formerly. He had recourſe to the

 medicine

medicine a ſecond time, ‘ but it produced ‘ ſuch violent heat in his ſtomach that he ‘ could not perſevere ; he continues how‐ ‘ ever much better than he uſed to be, ‘ and able to ride about his farm.’ ‘ I ‘ have,’ continues my correſpondent, ‘ given ‘ the pills in ſeveral more caſes of gravel ‘ in the kidneys, and always with ſucceſs.’ I have recommended for the laſt patient the ſolution with cream of tartar, and, if that ſhould fail, the impregnated alkaline water.

The following letter will be found particularly intereſting, as it affords hopes of relieving a very diſtreſſing complaint, for which as for ſo many others, we have no adequate remedy.

‘ *Shiffnal, June*, 1792.

‘ Dear Sir,

‘ I am very glad to hear of your inten‐ ‘ tion to publiſh your formula, with obſer‐ ‘ vations on the ſoda, but can contribute ‘ little to your ſtock of information upon ‘ the

'the ſubject. You are already appriſed of 'my opinion of the great uſe of vegetable 'and foſſil alkali in calculous concretions. 'No inſtances having occured to me, 'wherein its effects ſeemed to deſerve very 'particular remark, I am unable to offer 'more deciſive or more favourable evidence 'of this medicine, than what ſprings out of 'the general reſult of my experience. No 'opportunity has yet offered to me of try-'ing its power as a ſolvent of ſtone in the 'bladder; but in the nephritis calculoſa I 'have had abundant experience of its effi-'cacy, both in promoting the diſcharge of 'calculi with eaſe, and often in preventing 'their formation altogether. Yet amongſt 'the many favourable proofs which preſent 'themſelves, I muſt not conceal a few in-'ſtances, wherein the continued uſe of this 'remedy has, without any aſſignable cauſe, 'failed to procure relief. The proportion 'of theſe unſucceſsful caſes is but ſmall, 'and as an additional alleviation, I can ad-'duce ſome proofs of the utility of the

 'ſoda

'soda pills in relieving biliary concretions, 'which I know not if I have mentioned to 'you before, and if not, two of them you 'may perhaps think deserving the trouble 'of a short recital. In both cases the pa-'tients were females, and far advanced in 'years. In the first instance, the disease had 'existed upwards of fifteen years, and re-'turned pretty regularly at intervals of six 'or eight weeks, and sometimes oftener; 'the proxysms occurred with violent pain, 'which usually lasted many hours; the skin 'became deeply tinged with yellow, and 'after a day or two, a slight diarrhœa seem-'ed to relieve her. With some difficulty I 'persuaded her to try the remedy, which 'was made into pills with soap, and a few 'drops of oleum nucis moschatæ were 'added. Upwards of three years have now 'elapsed since she had any return of the 'symptoms. The other lady had suffered 'extremely from successive similar attacks, 'and was relieved about eighteen months 'ago by the same remedy, but is under the

'necessity

‘ neceſſity of a regular perſerverance in the
‘ uſe of it.

‘ Believe me, Dear Sir,
&c. &c.
‘ W. YONGE.’

Dr. BEDDOES.

Several queſtions of great difficulty and ſubtlety may be propoſed reſpecting the operation of alkaline ſubſtances in ſuch caſes as the preceding. 1. Do they merely produce the expulſion of concreted matter? or do they excite ſuch an action of the uropoietic organs, as tends firſt to produce, and then to expel this matter? I am diſpoſed to adopt the former ſuppoſition; ſince concretions are often lodged in the paſſages without exciting any pain, and ſince I have oftener than once witneſſed the diſcharge of ſmall calculi about the probable period of the beginning operation of the medicine.—Do alkalis act as lithontriptics or otherwiſe? In ſeveral caſes, as that of Bramah, they cannot be eaſily imagined to have any ſuch operation.

operation. The effects are much too sudden; the speedy and considerable discharge of gravel makes it probable that they produce, in the pelvis of the kidney or ureters, some movements, which observation shews to be by no means necessarily connected with an increased secretion of urine. When the patient feels this kind of irritation too sensibly, the dose of the medicine must be reduced. In some instances, solution seems really to have taken place, as in Case 1st, related by Mr. Jones; and many such are to be found on record. Our secretions may, it should seem, be altered, either by altering the action of the secretory organ, or by presenting to it materials different from those which it has been accustomed to work upon. The urine, by whatever cause, is remarkably changed: we are informed on very respectable authority, that the urine first made after a dose of mephitic alkaline water, 'will change turnsole paper to a blue 'colour, even if it be not taken above a 'quarter of an hour before the discharge of 'the

'the urine*.' I wiſh ſome other teſt of alkalis had been uſed: and ſtill more, that thoſe who have an opportunity of watching the progreſs of patients under a courſe of alkaline medicines would aſcertain—1. Whether the blood undergoes any determinate change.—2. Whether the urine becomes habitually alkaline.—3. Suppoſing this to be the caſe, and the patient to take vegetable alkali, is the exceſs of alkali in the urine, *vegetable*, or foſſil, or volatile? Many other equally curious points of inveſtigation would ariſe in ſuch an enquiry. Of all the ſecretory organs, the kidneys and the mammæ are moſt certainly and quickly affected by the paſſions and by food. By ſtudying the analogy that ſubſiſts between theſe organs, and by experiments not difficult to be contrived or executed, it is probable that we may acquire a conſiderable acceſſion of uſeful knowledge on a ſubject little underſtood.

* Account of the Efficacy, 3d edit. p. 117. Ordinary urine is known to be acid: Mr. Berthollet, the great Pariſian chemiſt, has obſerved the quantity of this acid to be much increaſed by certain diſorders.

Obſervations and Conjectures

On the SCURVY,

On OBESITY,

On PHTHISIS PULMONALIS,

AND

On CATARRHS.

IN one or two diſeaſes, the patient is apparently directed by a ſort of inſtinct to the means of cure: in all other inſtances, remedies muſt have originally been the pure bounty of accident. A few facts, however, would ſet ſpeculation to work; and in proportion as the number of ſubſtances capable of producing conſiderable changes upon the living ſyſtem increaſed, the data for anlaogical reaſoning in medicine were multiplied. It is, in fact, only by ſeizing thoſe new analogies, which are offered from time to time by diſcoveries in the phyſical ſciences,

 that

that we can hope to improve the art of medicine. Nor will any one, who will take the pains to comprehend this ſimple truth, require an apology for attempts to form new combinations of this ſort.

Mayow not only diſcovered ſeveral elaſtic fluids, and the eſſential properties of the moſt active of them all, but he aſpired to change the whole face of medicine and phyſiology, by the application of his wonderful diſcoveries to the appearances of animal nature. Immediately upon the revival of this branch of chemiſtry, Dr. Macbride attempted to derive the ſame advantage from it, and the minds of ſucceeding philoſophe have been engaged by ſimilar ſpeculations.

The two annexed papers of Dr. Girtanner afford an extremely ingenious ſpecimen of ſuch ſpeculations. His experiments are ſeveral of them happily imagined; that by which he has at once ſhewn the falſehood of the concluſion which Fontana had drawn from his laborious experiments on poiſons, is is conceived in a ſpirit far more philoſophical than that which has directed the greater number of experiments upon animals. His reaſonings are an attempt to inveſtigate the laws of organic bodies, by combining the medical opinions of Dr. Brown with Mr. Lavoiſier's theory of chemiſtry. He borrows little from Haller, beſides the term *irritability*, and if he has not mentioned the name of Brown, he has made a free uſe of his doctrines, and often employed his very expreſſions. He might not, perhaps, in an abſtract, think it neceſſary to point out the ſource from which ſo many of his ideas are derived, but in his larger work we have a right to expect that he ſhould do juſtice to departed genius.

The *Journal de Phyſique*, from which theſe papers are taken, is indeed abundantly known to the cultivators of ſcience, but it can by no means be ſuppoſed to fall into the hands of the majority of the practitioners of medicine in this iſland; and I was very willing to enchance the value of this little

 publica-

publication by annexing to it a train of reflections, in general well worthy of their notice, and calculated to excite their reflections. Mr. Woodhouſe, of the Middle Temple, at my requeſt, dedicated ſome hours of leiſure at Oxford to the tranſlation, and his knowledge of the French, and of his own, language, would probably have concealed from the reader that he has not made phyſiology his peculiar ſtudy.

For ſeveral years paſt I had been attempting to diſcover ſome part of the effects of oxygene air upon the animal œconomy: it appeared likely that its abundance or deficiency would ſenſibly affect the health, and that the chemical compoſition of the fluids and ſolids of the living body would influence their properties not leſs than the properties of dead matter, though not perhaps exactly in the ſame way. In ſome inſtances I thought I perceived as much certainty as either could be expected, or as is any where to be found in medical reaſonings, and in others there appeared a faint glimmering of probability,

probability, where total darknefs has hithereto prevailed. The fcurvy (fea fcurvy) I have long confidered as offering an application of the pneumatic chemiftry, nearly as direct and beautiful as the phænomena of refpiration; and it would be eafy to prove, by the teftimony of different perfons, that I had long fuppofed this difeafe to be owing to a gradual abftraction of oxygene from the whole fyftem, juft as death is produced in drowning, by withholding all at once the fame fubftance from that blood which is to pafs to the pofterior cavities of the heart. The proofs of this theory feemed equally fimple and ftrong; the livid colour of the blood, and the large livid fpots which are fo often fpread over the furface of the body, left little room to doubt of the abfence of oxygene; and the recovery of the fick, by the adminiftration of acids, and by a vegetable diet, afford a fort of confirmation fimilar to that which is derived from chemical fynthefis, for no fubftances are better calculated than acids at leaft, tc impart oxygene to the fyftem; they contain it in abundance, and they eafily part with it.

There

There muſt frequently be obſerved by thoſe who attend to the effect of evidence upon different minds, a ſpecies of intellectual cowardice, which refuſes its aſſent to juſt evidence, as long as a ſingle difficulty remains, though the facts conſtituting this difficulty do not oppoſe, any more than they coincide with, the theory. In hopes of clearing up ſuch difficulties, and applying the hypotheſis to the principal ſymptoms, I from time to time deferred the publication of an opinion, in ſupport of which I had collected various proofs.

In the mean time, the ſame theory occurred to a phyſician, whoſe acquiſitions and powers of reflection do him the more honour, as the greater part of his life ſeems to have been ſpent amid the hurry and incommodiouſneſs of a ſeafaring life.* Such an anticipation is very natural in the preſent

* *Obſervations on the Scurvy. By Thomas Trotter, M. D.* 1792. In the firſt edition of this ingenious treatiſe no ſuch theory is hinted at. The firſt edition was publiſhed in 1786.

period

period of ſcience, and however mortifying it may be to vanity, it affords the moſt powerful encouragement to perſevere in attempts to aſcertain the principles of the moſt important and moſt imperfect of all the arts, inaſmuch as it proves the poſſibility of reaſoning ſucceſsfully concerning a diſeaſe, which you ſcarce know but by deſcription.

Dr. Trotter, (from page 125 to 150) ſtates the leading circumſtance in this diſeaſe, the privation or diminiſhed proportion of oxygene in the blood. He quotes ſome of the fine experiments of Dr. Goodwyn, and mentions the exiſtence of oxygene in acids, which he juſtly imagines, they reſtore to the blood. He does not enter into any other particulars of theory. ‘ The *ratio-ſymptomatum*,’ he modeſtly obſerves, ‘ is ‘ certainly a difficult ſubject to enter upon. ‘ In what manner a diſeaſed ſtate of the ‘ blood communicates its influence to the ‘ moving powers of the body we are at a ‘ loſs to explain.’

It

It may be expected of a theory framed by two perfons independently of each other, that the fhades fhould differ, although the outlines be the fame. My reflections on the fcurvy have been more minute, and, in many refpects, my conclufions vary from thofe of this experienced writer. I fhall leave to the reader's confideration the two leading proofs ftated above, and fo largely expatiated upon by Dr. Trotter, and add fome mifcellaneous obfervations, which may tend ftill farther to eftablifh the theory. I fhall alfo frankly intermix my objections to fome of his opinions, chearfully fubmitting it to his ample experience to decide between us, in a full expectation that his regard to truth and utility will abforb all perfonal confiderations.

1. In the firft place, it is not quite accurate to impute the difeafe to a deficiency of oxygene in the blood. The deficiency is, doubtlefs, general to the whole fyftem. The difcolouration of thofe of the folids, of which the colour evidently depends upon

oxygene diſtributed to them from the blood, beſpeaks a deficiency here alſo. This diſcoloration is noticed in moſt of the accounts we have of ſcurvy. 'The gums 'have an unuſual livid appearance,' ſays Lind (3d edition. p. 100). In one paſſage the heart is ſaid to be white and putrid, the lungs blackiſh and putrid (p. 240); in another the muſcles are deſcribed as mortified (p. 242), and as falling to pieces on being handled. Dr. Blane could not diſcover, upon diſſection, any effuſion of blood *, where the livid ſpots appeared. Hence too it may be concluded, that the hardneſs and ſtiffneſs of the muſcles and tendons, a ſymptom generally obſervable in ſcurvy, depend upon the abſence of oxygene.

As oxygene is neceſſary to the contraction of the muſcles, it is probable that it is conſumed, or, more properly ſpeaking, enters into ſome new combination, in

* Obſervations on the Diſeaſes of Seamen, 1789, p. 515.

confequence of which it is eliminated out of the body; for we cannot but fuppofe that the quantity employed correfponds to the vaft quantity imbibed. Hence we may underftand the final caufe of quickened refpiration, during great exertions of the mufcles. This feems alfo to explain an obfervation which has fo frequently been made at fea, that the fcurvy makes its appearance after a ftorm, when the feamen, having undergone violent exercife, have expended a great part of the oxygene of the folids.

2. One of the moft pleafing, and, if I conceive it juftly, of the ftrongeft poffible arguments, may be drawn from fome appearances in the thorax after death. Dr. Goodwyn found, that in fuffocated animals the left cavities of the heart are full of venous blood. I have had abundant opportunities of verifying the truth of this obfervation. The death of fcorbutic patients frequently feems to enfue, in confequence of the blood being fo deftitute of oxygene, as to be incapable

capable of ſtimulating the left auricle and ventricle of the heart: perhaps alſo the inirritability of the muſcular fibres of the heart itſelf may contribute to this effect. The following paſſages will eſtabliſh the fact. 'The
'cavities of the heart were quite full of *cor-*
'*rupted* blood.' Lind, p. 240. 'All thoſe
'who died ſuddenly, without any viſible
'cauſe of their death, had,' according to another account, 'the auricles of their
'heart as big as one's fiſt, and full of coa-
'gulated blood.' p. 241.

Dr. Lind, from his own diſſections, mentions' that 'in patients, whoſe deaths were
'unexpected and ſudden, and where no
'effuſion of blood could be perceived into
'any cavity of the body, the heart was
'commonly much diſtended with blood;
'the auricles and ventricles of both ſides
'were filled, but thoſe on the right in the
'greateſt degree.' p. 503. This phænomenon would engage Dr. Lind's attention the more, as he was aware that though the right ventricle after death is generally diſ-

tended with blood, 'the left feldom con-'tains any.' p. 503.

This want of ftimulating power in the blood, and of irritability in the heart, will perhaps account for the fymptoms fo often mentioned, anxiety, tightnefs of the breaft, difficulty of breathing.

3. Dr. Trotter confiders, 'a deficiency 'of *recent* vegetable matter *alone*, as the 'occafional caufe of the fcurvy.' p. 171, 172. Yet we are certain that the blood, in the firft inftance, and afterwards the folids, are oxygenated by means of the lungs. They may acquire this principle by means of the ftomach; but we have no direct experience of their doing fo. It is only an inference from the compofition of acids and vegetables, and from their effects in the fcurvy. Between the reception of any given food into the ftomach, and the oxygenation of the blood by that food, there muft intervene unknown proceffes. It appeared therefore probable to me, that as feamen

feamen in general breathe an air containing a fmaller proportion of oxygene than any other defcription of perfons, the fcurvy might often originate from this caufe, and may be prevented or cured by guarding againft it; for whether we oxygenate the blood by the lungs or the ftomach, a difeafe depending on the want of this principle may, one would fuppofe, be equally obviated; nor would it be eafy in the prefent ftate of our knowledge to affign any circumftance that diftinguifhes the two cafes, except the heat fupplied by oxygene, when prefented in the ftate of an elaftic fluid, though it would be rafh to deny that other differences may exift. Captain Cooke's unexampled fuccefs in preferving his crews from the fcurvy during his two laft voyages, feems in great meafure owing to his extreme care to keep his fhips well aired. On many occafions they were reduced to falt provifions, and much longer out of fight of land, than many other fhips, which have been dreadfully afflicted with the fcurvy; in his laft voyage there never

never appeared among his crew any ſymptom of this diſorder; and in his ſecond only one man had it in any conſiderable degree.

4. It is extremely difficult to find ſuch preciſe facts as ſhall amount to an *experimentum crucis*, eſpecially as obſervation has not yet been guided by this theory. But Dr. Trotter has himſelf furniſhed an important obſervation, from which, if any one were to decide between theſe two cauſes of ſcurvy, want of freſh vegetables, or want of air ſufficiently furniſhed with oxygene, he muſt, I think, decide without heſitation in favour of the latter; and here I appeal to Dr. Trotter againſt himſelf. In July 1783, a ſhip, of which he was ſurgeon, arrived at Cape La How, on the Gold Coaſt of Africa. In the ſpace of a week above 'an hundred prime 'ſlaves, young, ſtout, and healthy,' were purchaſed. The competition however of the purchaſers at Anamaboe, whither this ſhip afterwards ſailed, ran ſo high, that in February, 1784, it had not on board two thirds

thirds of its complement. An indifpofition now began to prevail among the flaves, which foon afterwards proved to be the fcurvy; and before the arrival of the veffel at Antigua, of near fix hundred and fifty, near fifty had died, and about three hundred were tainted, in different degrees, with the fcurvy. Before they quitted the coaft, feven or eight had died, and between feventy and eighty were ill.

1. Of thefe flaves, 'the food confifted 'of beans, which were brought from Eng-'land, and rice and Indian corn, which 'were bought on the coaft. Thefe arti-'cles were boiled to the confiftence of a 'foft pafte, and made as near as poffible 'like the food of the country, by the ad-'dition of palm oil, Guinea pepper, and 'common falt—they were allowed to drink 'what water they pleafed.' p. 52.

2. They were 'confined below fixteen 'hours out of twenty-four, and permitted 'no exercife when upon deck.' (ibid.) 'The

'rooms,

'rooms, where they are ſecured below, are 'from five to ſix feet in height. They 'are ſtowed *ſpoonways*, and ſo cloſely 'locked into one another's arms, that it is 'difficult to move without treading upon 'them. The rooms are imperfectly aired 'by gratings above, and ſmall ſcuttles in 'the ſide of the ſhip, which, of curſe, 'can be of little uſe at ſea. The gratings 'are alſo half covered when it blows hard, 'to keep out the ſalt ſpray or rain. The 'temperature in thoſe rooms, when they 'become crowded, was above 96° of Fah-'renheit's ſcale.—I myſelf could never 'breathe there, unleſs under the hatchway. 'In ſuch ſituations it may be ſuppoſed, 'that the ſufferings of theſe creatures are 'ſometimes dreadful. Air, heated and 'rarified to ſuch a degree, and loaded with 'animal effluvia, cannot fail of being nox-'ious to life. There were certainly in-'ſtances where ſome expired from ſuffo-'cation, having ſhewn no previous ſign 'of indiſpoſition.' p. 54, 55.

3. With these two facts, let the reader compare the following: 'Few of the 'boys had any scorbutic symptoms; none 'of them were shackled; and by being 'allowed to run about the deck, and occa- 'sionally assist in the duty of the ship, 'their health seemed to be preserved by 'the exercise. This was also the case 'with the women, for out of the whole 'number eight only were affected.' (p. 63.) 'During this sickly state of the ship, none 'of the sailors were in the least tainted 'with the scurvy. Their diet was the 'common sea fare; a little of the victuals 'prepared for the slaves was generally eat 'with the salt beef; *they had it however in* '*their power* * to barter some of their pro- 'visions with the natives for fresh vegeta- 'bles.' p. 64.

The following is the inference which they author draws from these facts: 'I am ex- 'tremely unwilling to admit the conta-

* Did they all make use of the opportunity?

'gious nature of this diſeaſe; and if it 'cannot be propagated that way, it is 'moſt likely that the tainted atmoſphere 'of the ſlave rooms, which were now full, 'ſo powerfully prediſpoſed thoſe late pur-'chaſed negroes to ſcurvy, that the ex-'citing cauſe was much accelerated in its 'operations, by the foul air which they 'breathed; impure exhalations have there-'fore been deſervedly mentioned by au-'thors among the *remote* cauſes of the 'ſcurvy.' p. 68. For my part, I ſhould preſume that the diet of theſe unhappy people could have no ſhare whatever in producing the diſeaſe, which delivered ſo many among the men from their preſent and impending calamities.

5. There are other facts which ſeem to ſhew that too much is attributed by Dr. Trotter to *freſh* vegetables. Linnæus informs us, that the Laplanders are unacquainted with the ſcurvy; they feed all the winter on the freſh fleſh of the rein-deer. 'This exemption of the Laplanders 'from

'from the general diftemper of the north,' fays Pringle, 'is the more obfervable, as 'they feldom tafte vegetables, bread never.' (Cook's Voyages, from 1772 to 1775. vol. ii. p. 376.)

Confidering frefh meat, or the mufcular part of animals, chemically, I fee no reafon why it fhould not be efficacious in preventing or curing the fcurvy. Oxygene it contains, when *raw*, in a ftate of loofe combination, though probably not in fuch large proportion as vegetable fubftances, even fuch as are not acid. I had noticed in travellers of great refpectability paffages that confirm this idea. The nations inhabiting the cold and dreary regions on the eaftern fhores of Afia, and the oppofite coafts of America, feem to have learned from experience, that frefh, or at leaft unfalted fifh is a preventive of the fcurvy, or a remedy for it. Thus Dr. Pallas (Reife, iii. 47.) defcribes the Oftiack Tartars of the Oby, as preparing their winter ftores altogether without ſalt. ‘ They are extremely apt, ‘ when diſabled by age or infirmities, to be- ‘ come ſcorbutic. In winter they ravenouſly ‘ devour their frozen fiſh *raw*, a practice ‘ which the neighbouring Ruſſians imitate, ‘ eſteeming them a preſervative againſt the ‘ ſcurvy.’ (46.) Mr. Meares (Voyage, Introd. p. 30.) ſpeaking of an American tribe, ſays, ‘ She made us ſenſible that the ſame diſ- ‘ order (ſcurvy) prevailed in her nation ; ‘ and that whenever the ſymptoms ap- ‘ peared, they removed to the ſouthward, ‘ where the climate was more genial, and ‘ where plenty of fiſh was to be obtained, ‘ which never failed to prove the means ‘ of their recovery.’

The reader will probably agree to conſi- der the frozen as freſh fiſh. If it were poſſible to preſerve meat on ſhip board, in this ſimple manner, one great ſource of the ſcurvy would probably be cut off. Cookery combines the oxygene anew ; would our ſailors eat *raw* animal food ?

Dr.

Dr. Lind, though he has full confidence in green vegetables, and affirms that the ſcurvy never can ‘ become a general, fa-‘ tal, and deſtructive calamity,’ where they abound, and the proper method of treatment is known and purſued (p. 541.); concludes from a number of comparative trials (p. 538), that certain patients in Haſlar hoſpital in general grew better, notwithſtanding they abſtained altogether, from vegetables. ‘ This ſtrict abſtinence from the ‘ fruits of the earth,’ ſays he, ‘ was conti-‘ nued long enough to convince me, that ‘ the diſeaſe would often, from various cir-‘ cumſtances, take a favourable turn, inde-‘ pendent of any diet, medicine, or regimen.’ We have nothing, I preſume, to oppoſe in point of concluſiveneſs to ſuch experiments made by a phyſician ſo intelligent and ſo experienced in this particular diſeaſe.

The following caſe, for which we are indebted to Dr. Sandifort, Profeſſor of Anatomy at Leyden, is among the moſt extra-

extraordinary in the records of medicine. Its value however exceeds its ſingularity. Dr. Goodwyn quotes it in ſupport of his doctrine of reſpiration. It no leſs corroborates the foregoing theory. After the local ſymptoms ariſing from the conformation of the heart are ſet aſide, there will remain the principal characteriſtics of ſcurvy, livid ſpots or blotches, a bloated countenance, hæmorrhages, exceſſive laſſitude, diſtreſſing anxiety, frequent faintings upon ſlight motion, and a very offenſive breath, without an impaired appetite, and with a tendency to ſalivation. Diſtinct as the account is, I wiſh for ſtill greater minuteneſs, in a firm perſuaſion that every additional circumſtance, by affording a new analogy, would tend ſtill further to ſhew that the ſymptoms of ſcurvy ariſe from a deficient ſupply of oxygene to the blood by way of the lungs. For nearly the firſt year of the life of the infant in queſtion, who was born November 17th, 1764, there was no appearance of diſeaſe. The parents were healthy; the child was put out to nurſe; he he grew apace; ‘ but was ſcarce a year ‘ old *, when thoſe dreadful ſymptoms, ‘ which ſo much harraſſed him during the ‘ ſubſequent portion of his life, manifeſted ‘ themſelves. The livid colour of his ‘ nails and fingers firſt drew the attention ‘ of the parents. This hue was not con‘ ſtant; it was not occaſioned by any ‘ tightneſs of dreſs; it did not at firſt ap‘ pear of ſuch conſequence, as to induce ‘ them to call in a phyſician, eſpecially ‘ as the child ſeemed in other reſpects ‘ healthy, and made ſuch progreſs as to be ‘ able, by the end of the ſecond year, to ‘ walk alone.

‘ He now began to complain of great ‘ laſſitude upon the leaſt exertion. A ‘ catarrh, accompained with a violent op‘ preſſive cough came on. The child ob‘ ſtinately refuſed the medicines preſcribed ‘ by the phyſician, who was now called ‘ in, and the next day a number of ſpots,

* Obſervationes Anatomico-Pathologicæ, Lugd. Batav. 1777. p. 11, & ſeq.

which

'which at firſt were of a red colour, and 'then turned livid, were obſerved upon 'the face. The cough continued trouble-'ſome; motion became exceedingly diſ-'agreeable; and after any fatigue, the 'face, hands, and feet appeared remark-'ably livid; the tongue and lips were 'nearly black, but the natural colour re-'turned, upon remaining quiet ſome time. 'Theſe alternations were viſible almoſt 'every day. Meanwhile the child grew 'rapidly; the appetite was very good, and 'there was no complaint, but of laſſitude, 'preſſure on the top of the head, anxiety, 'eſpecially in winter, and ſuch chillineſs, 'even internal, that in winter he could 'not keep himſelf warm by the fire-ſide; 'nor did the rays of the ſun, on the hotteſt 'days, produce their natural effect, much 'leſs occaſion any ſweating.

'March, 1767. Bleeding ſeemed to 'leſſen the anxiety and ſenſe of preſſure 'for a time. The blood was thick and 'black, and no craſſamentum ſeparated.

‘ Towards the cloſe of this year he had ‘ the ſmall-pox mildly, and without any ‘ aggravation of his anxiety; nor did the ‘ meaſles and chicken-pox, which he ‘ caught a few months afterwards, produce ‘ any change either for the better or the ‘ worſe. The ſymptoms above mentioned ‘ attacked him the moment he moved, ‘ eſpecially the anxiety, which was accom- ‘ pained with ſuch violent palpitation, that ‘ the ſtrokes of the heart could be ſeen, ‘ and even heard. Riding which was now ‘ recommended, proved of no ſervice, nor ‘ could he bear it long at a time. ’

Gaubius, being conſulted in 1769, adviſed cold bathing and rubbing the body, but to no purpoſe; bleeding, and gentle motion were equally ineffectual; the anxiety, when extreme, was attended with a dry cough. ‘ The breath had an un- ‘ common ſmell, much reſembling that ‘ of an egg opened immediately after ‘ boiling.

‘ This diftreffing fituation continued till ‘ 1774, by which time the anxiety and ‘ violence of the palpitations were much ‘ increafed, and faintings, pain, like pref- ‘ fure, in the head, a fwelling and pulfation ‘ of the jugular veins, were obferved. ‘ Riding being utterly impoffible, frequent ‘ airings in a carriage were fubftituted in ‘ its ftead. In May bleeding diminifhed ‘ the anxiety; and he could now walk ‘ about for an hour, without any great fa- ‘ tigue, and his parents began to entertain ‘ hopes of his recovery. But all the fymp- ‘ toms recurred with greater violence in ‘ autumn; he coughed violently, and fpit ‘ up, fometimes mucus ftreaked with blood, ‘ fometimes pure blood. For this he was ‘ twice bled in November, within four ‘ days but the anxiety ftill continued, and ‘ fometimes rofe to fuch a pitch, as to ‘ threaten immediate death. The child ‘ was fenfible that he fhould not long fur- ‘ vive, he often remarked that his diforder ‘ was quite unknown and incurable, and

‘ that

'that no perſon could conceive what he 'felt about the heart. Another bleeding 'in December afforded a tranſitory relief; 'but the ſetting in of winter, a ſeaſon al-'ways intolerable to him, excited the ut-'moſt alarm in the parents.'

During the following year, the complaints continued equally diſtreſſing. In 1776, 'all motion became impoſſible; on 'the ſlighteſt exerciſe, he would faint, 'diſcharge a great quantity of ſaliva from 'his mouth, and continue blind for a time; 'all attempts to relieve him were vain; 'the anxiety was much increaſed, eſpe-'cially on laying down; what had formerly 'amuſed him now became indifferent; his 'face was bloated; his feet became œdema-'tous; yet his life was protracted in miſery to 'the 8th of March, 1777, when, upon being 'ſeized with exceſſive anxiety, he died.'

Dr. Hahn, who attended him during the laſt year, communicated to Dr. S. the following particulars:

'Immediately upon the very firſt glance, I 'recognized the complaint of which I had 'heard ſo much, and which report, as 'uſual, ſeemed to have exaggerated: the 'child was aſthmatical; on the ſlighteſt 'motion he breathed with ſuch difficulty 'that his face and hands became as livid 'as in a ſtrangled perſon; ſometimes they 'looked as if painted blue.

'The cauſe of ſo ſevere and ſo long 'continued an aſthma was obſcure; nor 'did it appear certain when the diſeaſe 'began. The parents, and the phyſician 'who had hitherto attended him, agreed 'in aſſuring me, that he was born healthy, 'and that no ſigns of complaint in the 'cheſt had occurred during the firſt year; 'nor was it till the ſecond year that the 'blue colour and ſymptoms of aſthma had 'been obſerved; even then the complaint 'was not conſtant, but became worſe and 'worſe, as the patient advanced in years 'and ſize.

'The

'The child ſeemed to me handſome, 'well-made, and tall for his years. He had 'a conſtant difficulty of breathing, which 'increaſed in cold weather, and upon mo-'tion; the face was bloated; the eyes 'were protuberant, fixed, and betrayed un-'eaſineſs. While the aſthmatic fit con-'tinued, his face appeared as it uſually 'does in a perſon who has been long walk-'ing apace againſt the wind in winter; 'the cheeks, point of the noſe, the ears, 'as well as hands, fingers, and nails be-'came livid; the lips, tongue, and inſide 'of the mouth were of a deep purple; 'the pulſation of the carotids was viſible 'at a diſtance; the pulſe at the wriſt very 'fluctuating.

'The child was ſenſible; his temper 'variable; but he was commonly moroſe 'and peeviſh; during his ſhort intervals of 'chearfulneſs, you could diſcover in his 'eyes and on his forehead, even while he 'ſmiled, a latent ſenſe of ſuffering; dif-'ferent complaints were perpetually re-

'curring,

'curring, as head-ach along the ſagittal 'ſuture, ear-ach, pain in the breaſt and 'left hypochondriac region, of ſickneſs, 'pain in the belly, extending as low as the 'os pubis, chillineſs, &c.

'At different times his ſufferings varied 'conſiderably. The following circum-'ſtances I conſtantly obſerved:

'1. Great dyſpnœa upon motion; viſible 'pulſations in the neck.

'2. A face too full for the habit of the 'reſt of the body; during his laborious 'reſpiration a livid colour of the coun-'tenance, a protuberance, and occaſional 'ſuffuſion of the eyes.

'3. Urine always high-coloured, with-'out ſediment.

'4. Great coſtiveneſs.

'5. Conſtant chillineſs, even though

'the

'the ſkin felt warm; this ſenſation never 'left him, except after he had become 'quite warm in bed. In winter, though 'ſitting cloſe by the fire, he complained of 'ſhivering, and in ſummer he longed for 'a large kitchen fire, and deſired to baſk 'in the ſun during the hotteſt part of the 'day.

'6. He was ſometimes much relieved 'by opening medicines and by hæmor-'rhages from his noſe, which happened 'from time to time.

'The tongue was very foul; the breath 'extremely offenſive (*fœtidiſſimus*).'

After quoting ſome caſes, not, I think, altogether in point, Dr. S. gives the following account of the diſſection. The thorax alone was opened.

'The pericardium did not, as uſual, 'appear ſurrounded by the lungs, and al-'moſt incloſed in them; but a maſs was

'ſeen

'ſeen to fill nearly the whole cavity, and 'to compreſs the lungs extremely: this 'maſs was the pericardium, containing the 'heart in a ſtate of great diſtention, and 'very full of blood. It reached from the 'diaphragm (which on the right ſide roſe 'to the fifth, on the left only to the ſixth 'rib) to the ſpace between the firſt and 'ſecond rib, and ſo entirely filled the late-'ral parts of the thorax, that only the an-'terior portion of the lungs on the right 'ſide (viz. the margin of the ſuperior 'and middle lobe), and but a very ſmall 'portion on the left (viz. towards the 'upper and lateral part) could be ſeen. 'Above the pericardum, the ſuperior 'cava, with the origin of the ſubcla-'vian veins, appeared turgid with black 'blood.

'Upon opening the pericardium, ſome 'water ran out; but not more, indeed not 'ſo much, as is ſometimes found in ſub-'jects where no dropſical ſymptoms have 'preceded death.

'The heart, when freed from its ſack, 'appeared exceſſively turgid, not however 'equably ſo: both the ventricles were not 'diſtended to the ſame degree; the right 'ventricle, as well as its auricle and ſinus, 'were much more enlarged, and full of 'blood, than the left; all the veins, which 'ramify from the coronary veins along the 'ſurface of the heart, were ſo dilated even 'to their extreme branches, that the moſt 'ſucceſsful injection could not have ren-'dered them more diſtinct.

'The veins ariſing from the ſubclavian, 'more eſpecially the jugular, were enor-'mouſly diſtended with thin black blood; 'the ſuperior vena cava, where it is 'lodged within the pericardium, did not 'much exceed its natutal ſize; the inferior 'cava was enlarged; the pulmonary veins 'were turgid, but not exceedingly ſo; 'the aorta was enlarged at its origin; the 'pulmonary artery was remarkably con-'tracted, from its origin almoſt to its bi-'furcation; of the arterious duct, or rather 'ligament, as it would have been at this 'age, there was no veſtige.

'The lungs externally had no morbid 'appearance; but they were ſmall, com-'preſſed, and not eaſily dilatable; whence 'it appeared that they could not properly 'have performed their functions.

'The external appearance of the heart 'ſhewed where the ſource of the miſchief 'lay. After tying up all the veſſels, it 'was ſubmitted to further examination.

'The right ſinus and auricle were firſt 'opened; a large quantity of thin black 'blood flowed out. In the foramen ovale 'there was an aperture, which would ad-'mit a large probe.

'On introducing the finger into the right 'ventricle, and turning the point towards 'the orifice of the pulmonary artery, where 'it uſually ariſes from this ventricle, no 'ſuch orifice could be felt, but it eaſily

'ſlided

'flided into another, and that a very large 'one. But how great was my aftonifh-'ment, and that of all the bye-ftanders, 'when it was difcovered, that the finger 'had paffed into the arota, which, accord-'ing to the ordinary law of nature, has 'no communication whatever with the 'right ventricle.

'This ventricle was divided in the 'place oppofite to the valve, behind which 'the arterious orifice lies, quite down to 'the apex. Upon lifting this valve a lit-'tle, the large mouth of the aorta ap-'peared, as alfo a fmooth margin; beyond 'which the finger found a way into the 'left ventricle of the heart. Upon cutting 'the aorta tranfverfely, at a proper dif-'tance from the femilunar valves, the 'fame margin was feen to divide its orifice 'into two parts, the larger communicating 'with the right, the fmaller with the left 'ventricle.

'The aorta therefore arofe from both

'ventricles, and muſt have received all the 'blood from both.

'The pulmonary artery having been 'cut acroſs above the valves, they ap- 'peared very ſmall, almoſt grown to the 'artery, and covered with a granular ſub- 'ſtance, reſembling a fleſhy excreſcence, 'ſo that only area enough was left to 'ſuffer a ſmall probe to paſs into the ven- 'tricle, and even this paſſed with greater 'difficulty from the ventricle into the 'artery. Upon opening the orifice longi- 'tudinally, we found only two ſhapeleſs 'valves, partly covered with the ſame gra- 'nular excreſcence.

'In the mouth of the arota there were 'three valves; in the left ventricle there 'was nothing remarkable, except the aper- 'ture in the ſeptum, and the thinneſs of 'its ſubſtance, which did not exceed, and 'indeed ſcarce equalled, that of the right 'ventricle.'

From

From the ſeveral particulars of this wonderful hiſtory and diſſection, the reader will be able to draw ſome importaint conclusions, beſides that already pointed out. 1. The comparative capacity and ſtrength of the ventricles in this caſe confirms Dr. Goodwin's diſcovery of the inirritability of the left ventricle by venous blood. Here the right ventricle ſeems to have performed almoſt all the labour of circulation: hence its enlargement, its equal or ſuperior thickneſs, the left not having, in conſequence of action, outſtripped it in this reſpect, as it does in healthy perſons; hence, probably, alſo, the larger opening of the aorta into the right ventricle. 2. This caſe removes all doubt as to the neceſſity of oxygene to the due action of the muſcles; a fact which the penetrating genius of Mayow perceived, though he miſconceived the mechaniſm of muſcular action, which we do not yet underſtand. 3. The Dutch phyſicians ſeem to have been ſomewhat at a loſs to account for the health enjoyed by the infant during the firſt year. But the full

full expanſion of the lungs is not neceſſary to life or health in early infancy, during which, for a ſhorter or longer period in different ſubjects, part of the blood paſſes through the foramen ovale and arterious duct, and is therefore not fully oxygenated *. Dr. Sandifort is doubtleſs right in concluding, that the ſtructure was connate; no rupture of the heart could have produced it. His information that the child ſcarce cried, never coughed, and was extremely quiet during the firſt year, deſerves notice. 4. From what has been ſaid above, the peculiar laſſitude felt on very high mountains, and deſcribed by M. Sauſſure † from

* Hence, probably, the *ſublivid* infantile complexion, which diſappears at various ages, juſt as the remains of the fœtal circulation ceaſe, and is perfectly diſtinct from the occaſional jaundice of infants.

† ' Les forces muſculaires s'epuiſent avec une extrême ' promptitude.—Ce qui diſtingue & caracteriſe le genre ' de fatigue que l'on eprouve à ces grandes hauteurs, ' c'eſt un epuiſement total, une impuiſſance abſolue ' de continuer ſa marche——On ne feroit pas á la lettre ' quatre pas de plus, fût-ce pour eviter le danger le ' plus

from his own ſenſations, may perhaps be ſatisfactorily explained. In this ſituation fatigue

' plus eminent.——Si l'on perſiſte a faire des efforts, on ' eſt faiſi par des palpitations & par des battemens ſi ' rapides & ſi forts, dans toutes les arteres, que l'on ' tomberoit en defaillance; ſi on l'augmentoit encore ' en continuant de monter.

' La ſeule ceſſation de mouvement, dans trois ou qua- ' tre minutes, ſemble reſtaurer ſi parfaitement les forces, ' qu'en ſe remettant en marche, on eſt perſuadé qu'on ' montera tout d'une haleine, juſques à la cime de la ' montagne. Or dans la plaine une fatigue auſſi grande ' ne ſe diſſipe point avec une telle facilité. Mr. ' Pictet ſe trouve tonjours faiſi d'une eſpece d'angoiſſe ' (anxiety), d'un leger mal de cœur, & d'un degout ' abſolu, deſqu'il eſt arrivé a la hauteur, d'environ 1400 ' toiſes au deſſus de la mer.' (Sauſſure Voyages, 4to. I. 482, &c.)

The profound ſleep (*aſſoupiſſement, ſomméil preſque lethargique*) ſeems exactly the aſphyxia ariſing from an improper air. The laſſitude is only the firſt degree of this aſphyxia. The anxiety, palpitations, &c. are all kindred ſymptoms, and what almoſt demonſtrates that they are here imputed to the right cauſe is a ſubſequent obſervation of Mr. Sauſſure, who tells us, that on the Col de Geant, at 1763 toiſes above the level of the ſea, ' De charbon ne brûloit que d'une maniere languiſſante,

' & a

fatigue very ſuddenly comes on, and as ſuddenly goes off on reſt. Now in aſcending theſe rugged heights, the muſcular exertion muſt expend a great deal of oxygene, which the rarified atmoſphere will ſupply but ſcantily. Hence the neceſſity of ſuſpending the ſevere exerciſe to collect a ſtock of this principle, and hence we ſee why it is ſo ſoon exhauſted. The other ac-

' & a force d'être animé per le ſoufflet.' (*Journ. de Phyſ. Sept.* 1788, *p.* 209)

Among the deſiderata in medicine, few are, I think, more felt than greater choice in the means of procuring ſleep. Opium, and other diffuſible ſtimuli, in ſome caſes, increaſe the reſtleſſneſs; and in moſt caſes it would be better to induce ſleep by the abſtraction of ſtimuli, than by exhauſting the excitability. Upon this principle we could not have a better ſoporific than an atmoſphere with a diminiſhed proportion of oxygene air: ordinary air might be admitted, when the patient was once laid aſleep. Mr. Sauſſure repeatedly mentions the very ſound ſleep he enjoyed during the nights he ſpent in the high ſituation above mentioned. The cold of which people traverſing great heights complain, I partly aſcribe to the want of oxygene to ſupport animal heat. From the narratives I collect that the ſenſible cold is greater than the thermometrical.

cidents of reſpiration, which have been aſcribed to a loſs of equilibrium between the external and internal air, probably depend upon the ſame cauſe. Mr. Plantade, at the age of ſeventy, ſuddenly and placidly expired, beſide his quadrant, upon the heights of the Pyrenees. Had he been opened, the left cavities of the heart would, I ſuppoſe, have been found turgid with blood, and, had he been carefully obſerved, his countenance would have ſhewn ſigns of ſtrangulation. He died, in ſhort, I conceive, nearly as he would have done in the exhauſted receiver of an air-pump, with this difference, that the preceding fatigue had conſumed the irritability of his muſcles. Young animals bear the abſtraction of ſtimuli better than old ones; and there is, probably, a certain mixture of oxygene and azotic airs that would be fatal to old, but not to young perſons. The experiments of Mr. Sauſſure, Pini, and Reboul, concur in ſhewing, that, independently of its rarefaction, the atmoſphere of very elevated mountains contains a far ſmaller porportion of oxygene than that of the lower regions, eſpecially than that of the high vallies of the Alps.

7. Therefore the inhabitants of theſe great elevations, if there were any, ought, according to the theory laid down above, to be peculiarly ſubject to the ſcurvy. But as no perſons paſs their lives in ſuch ſituations, we cannot expect from this quarter any deciſive facts one way or the other. Nevertheleſs, the only inſtance I know of a number of perſons continuing long in thoſe inhoſpitable regions, does actually ſupply a probability in favour of this doctrine. Mr. Condamine, during the operations of the French academicians, upon the ſummit of Pichincha, was attacked with ſcorbutic ſymptoms*.

(*) Un de'entre nous (Mr. D'Arcet ſays Mr. Condamine is meant) commença a reſſenter des affections ſcorbutiques, les Indiens et les autres domeſtiques que eurent des tranchees violentes; ils rendirent du ſang. (Bouguer, Mem. de l'Acad. R. des Sciences, 1744

The

The advantage to be derived from a better knowledge of the ſcurvy is, as in other caſes, to increaſe our power of preventing and curing it. And here a thouſand projects preſent themſelves.

1. The neceſſity of keeping ſhips ſupplied with freſh air becomes ſtrikingly obvious. And it would be worthy of another Lind to prove the truth of the theory, by ſupplying his patients with air containing a more than ordinary proportion of oxygene, which, however, to be done ſafely, muſt be cautiouſly attempted.

I was deſirous to know whether the temperature of ſcorbutic patients differed from the common ſtandard, but I find no obſervation of this ſort upon record.

2. We muſt build our hopes of prevention, and more eſpecially of cure, upon the acids. The efficacy of the native acids of vegetables is ſo ſtrongly atteſted by different writers down to the moſt recent, as Mr.

 Drinkwater

Drinkwater (*Siege of Gibraltar*) and Dr. Trotter, that if an adequate ſupply could be provided and preſerved, no other means would be requiſite for the preſervation of ſoldiers and ſailors, whether only liable to the ſcurvy, or really attacked by it.

A full trial of the mineral acids, as they are called, eſpecially the nitric and vitriolic, ought, I think, to be made. The vitriolic elixir, the worſt poſſible form has never, even in the opinion of Pringle (l. c. 383.) been properly adminiſtered. Water, to which a ſmall quantity of the acid ſhould be added, would obviouſly be the beſt form. Had theſe obſervations been printed before thoſe of Dr. Trotter, I ſhould confidently have ſaid, that the vitriolic acid is decompounded in the ſtomach and bowels, and that the uſe of the nitric acid, as appears alſo from experience to be the caſe with the oxygenated marine acid, would require great caution, leſt in conſequence of its eaſy decompoſition it ſhould inflame the ſtomach. I read with no ſmall ſurprize in

his

his work (p. 147), that the nitric and ſulphuric acids, 'in whatever matter they are 'exhibited, paſs through the body pure 'and unaltered, as when taken, into the 'ſtomach.' He repeats the ſame aſſertion (p. 184.) with reſpect to the ſulphuric acid. It is however certain, that almoſt all animal and vegetable ſubſtances decompound theſe acids; and, in truth, Dr. Trotter has completely miſunderſtood the table in the *Methode de Nomenclature Chimique*, upon which alone he founds his opinion. In this table he ſays, 'are to be found thoſe 'bodies, of which oxygene is a *compound*,' (he means a conſtituent part) 'arranged according to the degrees of elective attraction. At the top of the column is water, 'next follows nitric acid, carbonic acid, 'ſulphuric acid, &c. and not till after the 'tartarous acid come the oxalic, gallic, 'citric, and malic acids; hence theſe acids, 'by being more eaſily decompoſed, or their 'radicals and the oxygene being confined 'in weaker degrees of attraction, they are 'acted upon by the powers of aſſimilation 'and

‘ and digeſtion of the human body; by ‘ which means they become ſubjected to ‘ the animal proceſs, and form new com- ‘ binations with our fluids.’ The latter part of this paſſage ſeems to conſiſt of mere words; at leaſt, the ideas are vague; and it is certain, that the French philoſophers have exhibited only a view of their new names in the column in queſtion, and not the comparative attractions of different baſes for oxygene, otherwiſe the acids muſt have ſtood in a very different order. My opinion of the decompoſition of vitriolic acid in the alimentary canal is founded upon the anſwer to queſtions aſked with a view to aſcertain this very fact. Perſons taking vitriolic acid I have found to be very ſenſible of the ſmell of ſulphur upon different occaſions.

Either therefore vitriolic acid ought to be efficacious, or ſome clear and ſatisfactory reaſon ought to be aſſigned for its want of power. The ſame obſervation I think applicable alſo to vinegar. Some obſervations

in

in Lind ſeem to favour this expectation, as where he ſays, that ‘ the relief obtained ‘ by bathing the legs frequently in a day, ‘ with warm vinegar, is both *quick and ſur-* ‘ *priſing*.’ Did he make a comparative experiment with warm water?

If herpetic eruptions, * as there is ſome reaſon to ſuppoſe, depend upon an abſtraction of oxygene, leſs than that which occaſions the ſcurvy, or rather perhaps upon a local deficiency of this principle, one may preſume, that vitriolic acid, the beſt remedy we poſſeſs for the former, will not be inefficacious in the latter complaint. And although I am not led, by the improper application of a common name to confound two diſeaſes clearly diſtinguiſhed by their ſymptoms, yet it may, perhaps, without abſurdity, be proſpoſed as a query, whether

* The only circumſtance I cannot reconcile to this ſuppoſition is the efficacy of cantharides in theſe eruptions. The efficacy of certain metallic oxids, applied externally, favours it extremely.

they

they do not approach each other by a number of intermediate gradations. To illuſtrate my meaning, as well as to countenance my queſtion, I ſhall produce an example of a diſeaſe, which may, I think, be termed the *ſcorbutic leproſy, or herpes;* between which diſeaſes again there ſeems to me no certain boundary. Dr. Pallas obſerved this ſcorbutic leproſy in more than one place in the Ruſſian dominions. It makes a much ſlower progreſs than the ſcurvy. Thoſe whom it affects are not ſenſible, during the firſt and ſecond year, of any conſiderable pain or weakneſs. The countenance appears livid, as if they were ſtrangled; livid ſpots, little elevated, and giving no pain, are ſeen in different parts of the body; an herpetic eruption breaks out on the chin; the ſpots gradually enlarge, and violent pains are felt in the limbs; the herpetic eruption at laſt overſpreads almoſt the whole of the body, and to the bare, dark red ſpots ſucceeds a ſcurf, conſiſting of long ſcales, among which ulcers frequently break out. Sometimes the ſcurf falls off, and the ſkin takes on a natural appearance. If a perſon happens to wound or bruiſe any affected part, foul, ſpreading ulcers appear, and often eat down to the bone. The fingers are particularly liable to ſuch ulcers, and they commonly fall off joint by joint. This loathſome diſtemper except in its very laſt ſtage, does not produce much emaciation or debility. The animal functions go on well. (Pallas, Reiſe, 302, 303.) The Tartars hold this, which they call, from the complexion of the patients, the *black* diſeaſe, to be infectious; yet the phyſician, who deſcribes it, ſaw a family, were the elder brother was firſt ſeized, the younger three years afterwards, the mother a year after him, while the wives of the two brothers, who had conſtantly lived with their huſbands, continued perfectly free. This obſervation will perhaps appear deciſive againſt the opinion of theſe poor people, eſpecially when we reflect how rapidly the logic of terror, among the ignorant in particular, haſtens to conclude,

 that

that ſuch and ſuch diſorders are catching. Dr. Pallas mentions a particular, in which this diſorder of the Coſſacks differs from the leproſy ; the affinity however is ſtriking.

Theſe Tartars are utter ſtrangers to agriculture. They inhabit a country, where the ſoil is impregnated with ſalt, and abounding in ſalt lakes. Their diet is fiſh, often ſalted when it is more than half putrid. About the fort *Jaiſkoi Gorodec* alone, watery fruits, as melons, and the *moſt indiſpenſable kinds of garden herbs*, are cultivated, but not by the Coſſacks, who muſt buy, if they will have them, and who, therefore, I ſuppoſe, ſupply themſelves, but ſcantily.

3. SOOINS, an acidulous preparation of oatmeal, by which, as Pringle informs us (l. c. p. 382.), one of captain Cook's moſt intelligent friends cured his ſcorbutic ſick on board, deſerves more attention than it has obtained. It is prepared by pouring hot

hot water upon oatmeal, and ſuffering it to ſtand till it has become ſouriſh. The decanted water is to be boiled down to the conſiſtence of a jelly. The efficacy of this fermented acid affords a preſumption in favour of vinegar; and it may, I ſuppoſe, be generally procured on ſhip board.

4. Nitre ſeems to deſerve attention in the cure of ſcurvy. It is doubtleſs decompounded in the *primæ viæ*, and capable of ſupplying much oxygene. Theſe fatal conſequences, which have ſometimes followed exceſſive doſes of nitre, have been, I imagine, owing to an inflammation of the ſtomach, occaſioned by the oxygene; and it is, probably, upon the direct or indirect action of oxygene, that the danger of expoſing wounds and certain parts of the body depends.

5. Sweet wort, or the *extract of malt*, I conceive, from its chemical compoſition, to have ſome, though inferior, antiſcorbutic virtues. And the great authority of captain

Cook, who calls it one of the *beſt antiſcorbutics yet found out*, of Mr. Patten his ſurgeon, and others, ſtrengthens the opinion which chemiſtry ſuggeſts. It is true, as Dr. Trotter obſerves, that Macbride's theory is indefenſible; it is true alſo, that had there been nothing erroneous in his theory, wort, the unfermented and unfermenting extract of malt, could have had no efficacy in ſcurvy; neither has it been of late pretended that wort will of itſelf completely cure the ſcurvy; but from the conſiderable proportion of oxygene contained in ſugar, it is probable that it will retard the approach, and check the ravages of the diſeaſe. Dr. Trotter, I conceive, has much too lightly pronounced, that 'the ſimple wort paſſes 'eaſily through the body, and that it un- 'dergoes no decompoſition in the ſtomach.' And, admitting this, does he ſuppoſe that none is abſorbed? that it tranſmitted no oxygene to the ſyſtem of thoſe hundred and thirty ſcorbutic patients, who took it for fourteen days, without a ſingle inſtance 'of ſickneſs, 'gripes, or purging,' occurring (Lind, p. 539.)

p. 539,) and who, though they were not cured by the wort, appear, as one may fairly conclude from Lind's expreſſions, to have grown better while they were taking it? It would, I think, be raſh and unjuſtifiable in the higheſt degree to lay aſide a preparation, at once cheap, eaſy, and palatable, and which appears to poſſeſs, in whatever light it is conſidered, powers of prevention by no means deſpicable. I am inclined to think much the ſame of ſour krout, notwithſtanding Dr. Trotter's objections.—The relation between a diet of ſalted proviſions and the ſcurvy, it is not eaſy to determine with preciſion. Such a diet is certainly not neceſſary to the production of ſcurvy; the facts already enumerated afford abundant evidence of the falſehood of this hypotheſis, and it has, of late, been generally abandoned. Nevertheleſs, ſalted meat may be capable of giving riſe to the diſeaſe, either by itſelf deſtroying looſe combination in which oxygene ſeems to be held by hydrogene and azote in the recent fibrous and cellular ſubſtance of organic beings, or by

ſuffering

ſuffering a combination to be deſtroyed, in which perhaps oxygene is not a great deal more firmly held than by the azote of the atmoſphere.

The diet, therefore, of beſieged garriſons will, perhaps, account for the production of the ſcurvy, though the ſoldiers may not breathe the pureſt air in their barracks. Priſoners, eſpecially thoſe taken in war, are frequently attacked by this diſeaſe; and if we recollect how conſiderable their numbers have ſometimes been, and how cloſe their confinement, we ſhall be at no loſs to underſtand why they ſhould be viſited by this dreadful calamity.

One obſervation on the means of prevention and cure I will add before I quit the ſubject of ſcurvy. Of all the ſubſtances that can at once be cheaply procured and long preſerved, the concrete acid of tartar ſeems by far the moſt promiſing; it is very grateful, and comes near to the citric acid.

THIS theory of ſcurvy ſeems, with ſome modification, capable of being applied to the accumulation, or, more properly ſpeaking, the ſecretion of fat in exceſs. The oily parts of animals, I believe, principally differ from the reſt of the heterogeneous maſs, commonly called fleſh, in a ſingle circumſtance; they appear to contain a ſmaller proportion of oxygene. The actual convertibility of the ſofter ſolids into a ſubſtance like ſpermaceti, is a clear proof of the affinity between muſcle, cellular ſubſtance, and fat, as to their compoſition; add or take away a little of one or more of their conſtituent parts, and the alteration is effected. The converſion, as one may, I think, infer from the obſervations made in the burying ground of the Holy Innocents at Paris *, depends on ſome very ſlight cauſe.

The

* In the large *common graves*, where ſeveral hundred bodies lie together, all the ſoft parts were wonderfully

The diſtillation of organic matter, violent as that operation is, favours the ſuppoſition.

fully changed; in ſome ſubjects, where the proceſs of converſion was not finiſhed, a few of the muſcular fibres were viſible; in the reſt, all the muſcles, even the heart, the membranes, tendons, and viſcera had diſappeared; the brain was alſo changed. The ſubſtance, into which all was alike changed, was ſoft, ductile, greyiſh white, much reſembling new cheeſe. It frothed extremely with water, and proved to be an ammoniacal ſoap, from which acids ſeparated a concrete oil, which when dried ſlowly, was like ſpermaceti, having a lamellated, cryſtalline texture; it was much more ſoluble in alcohol than ſpermaceti is, and it readily diſſolved in cold ammoniac. As it bore ſome reſemblance to wax, Mr. Fourcroy propoſes to call it *matière adipo-cireuſe.*— The ſoap or *gras* is ſaid, however, not to conſtitute above $\frac{1}{10}$ or $\frac{1}{12}$ of the body. The reſt evaporates in the form of water, carbonic acid air, and perhaps ſome unknown elaſtic fluids. (Annal. de Chimie, v. 154. viii. 17.)

Mr. Thouret appears to entertain ſome doubt as to the converſion. He ſays they appear to be converted, but imagines the converſion may not be real. He ſeems to be perplexed by ſome indiſtinct hypotheſis lurking in his mind, and perhaps is not aware that converſion can only mean a change from the loſs or acceſſion of ſome conſtituent part. Except the bones, the colouring matter of the bile, of the bronchial glands, the pigſition. The water and carbonic acid, having appropriated the oxygene to themſelves, the oils, ammoniac, and carbone appear to be formed from the reſiduary azote and carbone. From Mr. Fourcroy's experiments, as well as from the proceſs of diſtillation, it looks as if the production of animal oil and ammoniac were operations nearly allied. One would not aſſuredly reaſon with much confidence from diſtillation to ſecretion, eſpecially as the diſtilled

mentum nigrum, 'and, perhaps, the *proper* ſubſtance of 'the muſcles, this transformation has entirely ſubdued 'all the other parts; the ſkin, adipoſe ſubſtance, mem-'branes, muſcles, viſcera, cartilages, glands, tendons, 'aponeuroſes, and even the fluids.' Again—'though 'the transformation ſeems to have taken place in the 'muſcles, it has probably eſtabliſhed itſelf in them by 'means of the lymphatic and unctuous juices.' (J. de Phyſique, April 1791, p. 256—7.) Mr. Halle, in treating the fibrous parts of animals with nitric acid (ibid. Mai, p. 338—9.) obtained an oily concrete, ſimilar to ſpermaceti, and unalterable by nitric acid. If, as Mr. T. ſeems to argue, this were really the baſe of the animal fibre, it might eaſily happen that during a certain ſtate of the ſyſtem it ſhould be changed into fat inſtead of muſcular fibre.

differ in appearance from the ſecreted oils, and doubtleſs alſo, ſomewhat in the proportion of their conſtituent parts. But is it unlikely that, if the blood itſelf ſhould ever contain but little oxygene, a ſubſtance containing a ſmaller proportion of oxygene than other animal ſubſtances, ſhould be formed? and if muſcle, membrane, tendon be changed into fat after death, why may not the powers of life, inſtead of muſcular atoms, occaſionally combine into fat thoſe elements, which, under other circumſtances would have gone towards the formation of fibres or laminæ? In diſſection we avoid fat ſubjects, when we deſire to ſhew the muſcles diſtinctly. Is it that there is leſs muſcle as there is more fat? or are the fibres, the maſs of muſcle remaining the ſame, only rendered more indiſtinct? Every new hypotheſis, if it be but thought worth the trouble of refuting, adds to our ſtock of well-aſcertained facts. Till it be certainly decided, whether, as I ſuſpect, the fat encreaſes at the expence of the muſcle, let us try if we cannot diſcover, in the living ſyſtem itſelf, ſome obvious appearances.

pearances, indicating a tendency to form fat, whenever there is a deficiency of oxygene to a certain degree.

1. Among the Africans, of whofe fufferings on board the flave fhip Dr. Trotter has given fo particular and affecting an hiftory, corpulence feems to have been, as it were the firft ftage of fcurvy. 'When a 'negro was becoming rapidly fat,' fays he, 'it was no difficult matter to determine 'how foon he would be feized with the 'fcurvy;' fo that corpulence feems here to have been the harbinger of the fcurvy. Writers have been particular in noticing that this difeafe feldom or never produces emaciation. Dr. Trotter, upon whofe information we may place full reliance, tells us, that having purpofely enquired among his medical acquaintance in the navy, he did not find one who confidered the wafting of the flefh or abforption of fat as a fymptom 'congenial to fcurvy.' He immediately fubjoins fome obfervations of his own, that clearly indicate a connection between

the ſcurvy and obeſity (p. 98, 99.) 'In 'a corpulent ſtate of the body,' he ſays, 'the moſt hideous features of the diſeaſe 'are expreſſed; ſuch are the bloated looks 'and countenance, &c.' In a meſs of midſhipmen, who lived altogether on the ſhip's fare, the only one he ever ſaw affected with the ſcurvy was 'a young man remark-'ably corpulent.' From the whole of his obſervations it appears clearly that obeſity pre-diſpoſed his patients to ſcurvy, or rather was to them what cachexy is to dropſy.

2. The emaciation produced by acids, which is exceſſive, where they are taken to exceſs, is a fact of which it is only neceſſary to remind the reader. In cyder countries the people are habitually leaner than where beer is the common liquor.

3. That inactivity which ſtands to obeſity in the relation both of cauſe and effect, generally prevents fat perſons from attempting to reduce themſelves; and though they may make a few ſtruggles a firſt, they finally ſubmit

ſubmit to the incumbrance. They ſeldom conſider themſelves as in a ſtate of diſeaſe, and a prejudice prevails, that corpulence cannot be cured without danger. Hence we hear but little of the medical treatment of obeſity. The authentic facts, however, which we poſſeſs, ſhew that it may be ſucceſsfully treated in the ſame manner as the ſcurvy. This analogy is particularly ſtriking in two caſes of unwieldy corpulence, which Fothergill removed by a ſtrict vegetable diet. Mr. Wood, whoſe caſe is ſo well known from his own account publiſhed in the Medical Tranſactions, is another inſtance of the ſame kind.

4. Short-winded perſons are very often corpulent, and even many aſthmatics. Wherever the livid colour * of the countenance

* I mean an occaſional or prevailing hue, not a permanently fixed ſtain, ſuch as the red noſe and cheeks of perſons poiſoned by fermented liquors exhibit. This deformity I ſuſpect to ariſe from inflamed lymphatics, which, by continued exceſs in drinking, become indurated. When a red noſe alternates, as it often does, with pain in the liver, are the abſorbents only or the whole

nance indicates a deficiency of oxygene, there you will feldom fail to obferve a full habit. Here I fhall probably be told that I put the effect for the caufe, fince dyfpnœa is the confequence of the accumulation of fat. Inftances may alfo be produced of an emaciated habit of body, attended by difficulty of refpiration.

In anfwer to the firft objection one would not affert any thing pofitive; it is a fubject on which it is not eafy to make decifive obfervations. It only appears to me, from my remarks on corpulent habits, that dyfpnœa and obefity favour each other, that I think it evident alfo that every different fpecies of dyfpnœa, though in appearance

whole organized fubftance alike affected? My reafon for fufpecting the lymphatics to be principally affected is not only becaufe the career of grofs debauchery fo frequently terminates in dropfy, but becaufe I have been able to feel cords like fcirrhous abforbents in various parts of the bodies of perfons deftroyed by alcohol. Had fo much induration been owing to fcirrhofity, lancinating pains muft have been felt, which I have been affured by the patients was not the cafe: nor was there any caufe whatever befides thefe cords for fufpecting the prefence of fcirrhus or the approach to cancer.

equally

equally diftreffing to the patient, does not equally prevent the action of the air and the blood on each other.

It is not fo difficult to elude the force of the other objection. The living body is doubtlefs fubject to the influence of counteracting caufes, of which that which tends to diminifh the fat may prevail over its antagonift. Fat may be fecreted without being accumulated. In a perfect theory, thefe counteracting caufes, I am fenfible, ought to be fpecified, and their power eftimated; but we are fo far from having attained fuch precifion in the knowledge of any one of thofe various chemical operations that are comprehended under the improper term *fecretion*, that it is making fome advance to indicate a probable general caufe, even if we are obliged to leave its action to be accurately determined hereafter. I object to the term *fecretion*, taken in its proper fenfe, becaufe many circumftances concur to render it probable, that only the elements of *fecreted* fubftances exift in the blood, and that combination is the office of the

the fecretory organs. No one now, I fuppofe, will maintain that they are mere filtres.

5. The mechanical effect of exercife has never yet been fully appreciated. I have no thought of difturbing the abforbents in the poffeffion of their claims; their action is, I doubt not, increafed by exercife. But may not exercife alfo, by introducing more oxygene into the fyftem*, and by diffufing it more widely, check the formation of a fubftance containing little oxygene, while the fat, in common with the other fluids and folids, is abforbed? In the courfe of life there are two periods during which the fat is apt to accumulate; the firft is

* A very delicate obfervation made by Mayow is well deferving of notice in eftimating the effect of exercife. A dog that was panting and breathing deeply, on receiving arterial blood into one of his veins, inftantly began to breathe fo calmly that his refpiration was fcarcely fenfible. The animal received from an unufual fource the fubftance which is probably expended in mufcular action; it was therefore no longer neceffary to inhale it rapidly.

that of infancy, during which not inaction merely, but, if I am not deceived, the ſtate of the blood alſo, favours the formation in exceſs as well as the accumulation of fat. The lungs are not probably fully expanded for a conſiderable time after birth ; and whatever quantity of blood goes to the aorta by the arterious duct, or to the left auricle by the foramen ovale, which frequently continues open for a conſiderable time, muſt ſo far prevent the oxygenation of the ſyſtem*.

The commencement of the ſecond is fixed by Haller at forty years of age. At this period of life an indolent diſpoſition is coming on ; neither curioſity nor the other paſſions any longer agitate us with equal force. At this period I conjecture that there is alſo a deficiency of oxygene in the ſyſtem ; and the conjecture will receive

* Cæcum *ductum arterioſum* reperi aliquando poſt paucos menſes, alias ſerius, ut ſecundo anno abſoluto tubulus pervius fuerit, *Haller*. III. 161. celerius adeo quam foramen ovale clauditur, ib. 162.

ſome confirmation from the papers of Dr Girtanner; whether it be that the ſtomach and lungs are ſo altered as not to imbibe it in the uſual quantity, or the other conſtituent parts of the ſolids and fluids are ſo altered in their proportion as to have loſt ſomewhat of their attraction for oxygene, or upon whatever elſe it may depend. The leſſened vigour of the whole ſyſtem, the diminiſhed irritability of the muſcular fibre, the wearineſs that now ſo much ſooner follows exertion, indicate this, and the rigidity of the tendons, fibres, and laminæ may, in advanced age, be owing to a permanent, as, in ſcurvy to a temporary, deficiency of the ſame principle. If this ſuppoſition were juſt, might not ſome means be diſcovered to protract the period of youth and vigour indefinitely.—Whether true or falſe, and even though we ſhould never be able to reſtore new excitability to the ſyſtem, there can be no doubt of the immediate practicability of prolonging life conſiderably, and, what is much more deſirable, of maintaining a firmer ſtate of health, by a proper

management

management of the excitability during the periods of infancy and youth.

6. It is generally obſerved that perſons who indulge much in ſleep are apt to grow fat. From the well-known infrequency of reſpiration during ſleep, a ſmaller quantity of air muſt be taken into the lungs than while we are awake. The neceſſary conſequence and its application are obvious. The zoologiſts ſupply obſervations to the ſame purpoſe. Thus, *Mures montani hieme pinguiſſimi* fiunt, *etiam abſque cibo**; *idemque de urſis eſt notiſſimum* †; *ut recte pinguescant gallinæ, plurimum ut dormiant neceſſe*

* Haller Elem. Phyſiol. I. 39. For this fact he quotes Ray's *Wiſdom of God*, but, as I remember, erroneouſly. Ray, if I do not miſtake, ſays, that the marmot, the mus montanus here underſtood, is fat in autumn, and comes out lean in ſpring, juſt the contrary of what Haller makes him ſay. I do not therefore build upon what he aſſerts of the marmot or the bear: the other facts are to my purpoſe.

† Ariſtot. Hiſt. Anim. VI. 36. Hillerſtroem Iamtelands Diur. ſang.

 eſt,

eſt *, *eoque ſcopo ipſum lolium in cibum miſcetur* †.

Vegetables doubtleſs decompound water; it appears almoſt certain that they muſt combine the hydrogene with azote abſorbed from the atmoſphere, to which a certain portion of oxygene is added. How then comes it to paſs that theſe elements are not at times ſo combined as to produce an exceſs of oil and a ſort of obeſity in the vegetables? ſome ſuch modification of their functions does really take place. In proportion as they are more expoſed to heat and light, they ſeem to form a larger quantity of oils and reſins, as well as of ſaccharine matter, which is nearly allied in its compoſition to the two former ſubſtances. Wherever they are able to decompound moſt water, there alſo they will probably abſorb moſt azotic air. Muſt not theſe two functions go on moſt vigorouſly

* Reaumur, Art de faire eclorre des oiſeaux domeſtiques, p. 2. id. 2. p. 392.

† Id. p. 393.

vigorouſly in thoſe plants, which in the ſame ſoil and climate form moſt oil? We know beſides, that vegetables are capable of forming oils, either exactly the ſame as thoſe of animals, or very nearly reſembling them. Thus we have the ſuet of the *croton ſebiferum*, the butter of the *phœnix dactylifera* and of the *butyrum cacao*. When from a more intimate acquaintance with them, we ſhall be better able to apply the laws of organic bodies to the accommodation as well as the preſervation of life, may we not, by regulating the vegetable functions, teach our woods and hedges to ſupply us with butter and tallow? Thus our paſtures and meadows, the moſt fertile ſpots in every country, would, many of them, be gained to the cultivation of corn, the immediate food of MAN. And how many millions of inhabitants more might BRITAIN maintain, if we could feed upon the immediate produce of the ſoil? how many tons of vegetable food are condenſed into every fat ox?

WHEN

WHEN the medical practitioner finds himself deserted, in every emergency, by the hypotheses of the schools, he is too apt to attach himself to a blind routine, and to conclude that attempts to explain the capricious phænomena of diseases, for so they must appear to him, will be hereafter as fruitless as they have heretofore been. The next step is to decry the value of that which he neither possesses nor hopes to attain; nor will he tolerate in others attempts to discover the laws of animal nature, of the existence of which it is yet impossible to doubt. Perseverance in the observation and comparison of phænomena, which have hitherto been observed and compared in vain, is the privilege of a few superior minds. Examples of ill success in these difficult investigations do not overpower their firm persuasion of the existence of invariable laws, and of the possibility of a complete theory of diseases. I feel not much reverence for those who

pique

pique themſelves upon pure experience. There are few diſeaſes in which we have any fixed rule of practice; and our ſpecifics are ſo few, and ſo eaſily applied, that this part of medicine may be acquired without difficulty or loſs of time. In moſt inſtances a theoretical deliberation of ſome ſort muſt precede preſcription, and here the diſcrimination of perſons habituated to ſpeculation will have the ſuperiority of ſkill over chance, and their fertility of reſources will appear to peculiar advantage.

In the conſumption of the lungs, as indeed in too many other diſeaſes, a conjecture may be offered with the leſs diffidence, ſince experience cannot here ſet up the ſlighteſt pretenſion to overawe ſpeculation. This melancholy truth will, I hope, propitiate thoſe whoſe diſpleaſure might otherwiſe ariſe againſt an attempt to diſturb medicine by the introduction of new ideas. I beg leave alſo to aſſure perſons little acquainted with the recent progreſs of ſcience, that I do not employ the French chemical nomenclature

clature from affectation; but as the French has prevailed over the old theory of chemistry, so I expect that the terms sanctioned by the founders of that theory, however uncouth they may sound at present, will finally establish themselves, partly as being more convenient, partly as being the language in which the most eminent philosophers of Europe communicate their discoveries. Persons acquainted with the substances they design will find no obscurity in the terms, and for others it is indifferent what terms are used.

Not much seems to be gained by ranking the phthisical tumours and ulcers of the lungs among scrophulous complaints. We have no very successful method of treating scrophulous sores, wherever situated; neither have we any tolerably clear idea of the nature of the disease. Not to mention that very different ailments are comprehended under a term so conveniently vague. I see no hopes therefore of transferring any useful ideas from the external appearance of ſcrophula to the internal form of the diſeaſe, if they ſhould be eſſentially the ſame, of which I am by no means ſatisfied. We ſee, it is true, ſtrumous perſons attacked by conſumption; but we may obſerve many others attacked by it, who have had no glandular ſwellings, or other marks of ſcrophula. However this may be, the only circumſtance in phthiſis, from which, in our preſent ſtate of ignorance, we can hope to reaſon to any purpoſe, has always appeared to me to be the *occaſional* effect at leaſt of pregnancy in ſuſpending the progreſs of phthiſis; for if we could once diſcover how pregnancy produces this ſingular effect, we might be led to diſcover alſo a method of ſuperinducing and prolonging the ſame change of the ſyſtem at pleaſure.

I had repeatedly attempted to proceed through the obſcurity by the help of this clue, but in vain. I have lately had a very favourable opportunity of obſerving this effect of pregnancy, but could fix on no plaufible fuppofition, with which I might compare the phænomena. At laft, when it was too late, the difeafe having returned and deftroyed the patient, the following fuppofition occurred: The fœtus has its blood oxygenated by the blood of the mother through the placenta. During pregnancy there feems to be no provifion for the reception of an unufual quantity of oxygene. On the contrary, in confequence of the impeded action of the diaphragm, lefs and lefs fhould be continually taken in by the lungs. If therefore a fomewhat diminifhed proportion of oxygene be the effect of pregnancy, may not this be the way in which it arrefts the progrefs of phthifis; and if fo, is there not an excefs of oxygene in the fyftem of confumptive perfons? and may we not, by purfuing this idea, difcover a cure for this fatal diforder?

I am not able to prove the exiftence of this *phlogifticated* or *putrefcent* ftate of the fyftem, as it would formerly have been called, during pregnancy. I muft therefore

appeal

appeal to the preſent knowledge or future obſervation of accoucheurs to determine whether it manifeſts itſelf in the colour of the blood, bleeding of the gums, dark coloured ſpots, vibices, or any other ſcorbutic ſymptoms. The ſtate of the blood in pregnancy is certainly peculiar; this the writers on midwifery declare in very pointed terms; but I have not found its ſenſible qualities ſo well defined as might be wiſhed. Thus we read that 'the blood of pregnant 'women is always found to have what is 'called a ſizy appearance, though of a peculiar kind, and evidently very different 'from that which is obſerved in caſes of inflammation *.' I know not whether the ſpots or blotches mentioned by the ſame author †, or the red ſpots ‡ mentioned by Mauriceau as covering the legs and thighs, or the dark colour of the abdomen and inferior extremities deſcribed by Camper in his diſcourſe on the colour of the negroes, as often ſeen

* Denman, Introd. to Midwifery, p. 248.

† p. 266. ‡ p. 72.

in pregnant women, which Le Cat had alſo obſerved, are ſcorbutic blotches; and if ſo, whether they proceed from a conſtitution habitual during geſtation, and in theſe inſtances more ſtrongly marked than uſual. This conſtitution, if it exiſts at all, requires nicer attention than has probably been paid to it, both becauſe it is not likely in general to recede very far from the healthy ſtate of the ſyſtem, and becauſe ſome phænomena are ſeldom well obſerved, till they are obſerved with reference to an hypotheſis. The natural effect of an habitually ſtraitened reſpiration will have more weight with the ſkilful and reflecting reader than this lame and defective evidence. The following coincidence he will perhaps think remarkable. Pregnant women agree with ſcorbutic patients in that ſtrong inſtinctive appetite for vegetables, and it appears as if this diet was the moſt ſuitable to them. 'Pregnant wo-'men,' ſays Dr. Denman, 'have generally 'a diſlike to animal food of every kind, and 'under every form—on the contrary, they 'prefer vegetables, fruit and every thing 'cooling,

'cooling, which they eat and drink with 'avidity, and in which they indulge with- 'out prejudice *.'

But is the inference from the ſuppoſed deficiency of oxygene in pregnant women confirmed by appearances about phthiſical patients? Do they ſhew any ſigns of a redundance of this principle?

The clear, bright, and florid hue of the fluſhed hectic countenance, ſo diametrically oppoſite to the ſcorbutic complexion, affords ſome preſumption of a ſtate of the blood, equally receding, but in an oppoſite direction, from the ſtandard of health. The countenance of perſons fluſhed by exerciſe or food widely differs from that vermilion bloom, which is miſtaken by the uninſtructed for the ſign of health, though it is the harbinger or attendant of an incurable diſeaſe †. On holding the hand of a conſumptive

* Introduct. to Midwifery, p. 249.

† The clearneſs of ſkin in perſons ill of phithiſis, or pre-

ſumptive perſon againſt the light, the ſemi-transparency of the margin of the fingers and joints is, I think, evidently of a more vivid carnation in conſumptive patients. A ſtriking difference would probably be perceptible between the hand of a phthiſical aud ſcorbutic patient. In the former, the appearance may, indeed, be imputed to emaciation; but there are ſimilar appearances which cannot by any means be imputed to emaciation, nor, in my opinion, to any cauſe except that aſſigned above. 'During the fe-'ver,' ſays a late attentive writer, who did not here look through the coloured medium of hypotheſis, 'the cheeks appear as if 'painted with a circumſcribed ſpot of *pure* '*florid red;* the lips and the tubercles in

pre-diſpoſed to it, is a general obſervation. *Qu.* When it attacks perſons who have herpetic eruptions, do thoſe eruptions ceaſe?—Is not the chillineſs of ſcorbutic patients, and of the *blue boy*, owing to a want of oxygene? this chillineſs ſeems prefectly diſtinct from febrile horror and rigor.—And is not that *ſenſe* of heat, which ſo much diſtreſſes phthiſical patients, owing to *actual* heat, produced by the exceſs of oxygene?

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'the canthus of the eyes are alſo redder 'than when in health *.' Again, towards the termination of the diſeaſe, 'the tongue 'appears clean, and, with the fauces, is of a '*bright red*†.' Here I requeſt every reader acquainted with the modern doctrine of reſpiration (otherwiſe he is not a proper judge) to pauſe a moment. It will, I imagine, readily be allowed that if the blood-veſſels were filled with more florid blood than uſual, this identical bright redneſs would be ſeen. The cauſe, then, is adequate to the effect: the change, too, is not, as in the looſe analogies of the humoral pathology, tranſported from ſome remote part of inanimate nature, and at random imputed to animals. If the various evidence adduced in the caſe of ſcurvy ſhould not prove ſatisfactory, yet the moſt determined ſceptic will not heſitate to admit in aſthma a deficiency of oxygene and a correſponding change of colour. Why then will he not infer from an oppoſite change of colour an

* Reid's Eſſay on Phthiſis Pulm. ed. 2nd, p. 13.
† Ibid. p. 19.

oppoſite

oppoſite condition of the blood? If in the *puer cæruleatus* of Dr. Sandifort, the tongue and fauces were deep blue or purple, becauſe the lungs did not admit oxygene enough, will it not be granted that a bright red indicates the reception of too much?

'But then the blood itſelf ought, according to your ſuppoſition, to be brighter than ordinary;' the arterial blood certainly; and a compariſon of the arterial blood of phthiſical and healthy perſons would be ſo intereſting on this very account, that I wiſh it were practicable. But if we conſider the whole action of oxygene upon the blood, we ſhall not perhaps expect the difference to be ſenſible on a compariſon of the venous blood; for oxygene ſometimes darkens and ſometimes enlivens the venous blood; not, however, indiſcriminately, but under determinate circumſtances. When blood is expoſed to oxygene air, it firſt becomes florid, and afterwards black. Mr. Fourcroy and Mr. Haſſenfratz * have aſcertained this

* Annales de Chimie, t. IX.

fact in a very careful manner; and I have had several opportunities of remarking the same change. The oxygene, which at first is combined with the whole mass of the blood, or the red globules at least, after some time is attracted by the hydrogene alone or united with azote, and forms accordingly water or carbonic acid. If oxygene be added in a large quantity at once, or loosely combined, the blood is never brightened, but turns immediately black, as when oxygenated marine acid is added to blood.

Probably the solids, during circulation, more than divide with the blood its loosely attached oxygene; if they have a superior attraction, they will, as some of the constituent parts of the blood itself do upon standing, take the whole and leave the blood dark-coloured.

Hence then it appears possible enough that an unusual quantity of oxygene being thrown in through the lungs, the solids might attract it and be consumed them-

 selves,

ſelves, or prepared for abſorption in ſome way not underſtood, while the venous blood returns to the lungs of its ordinary colour; and, were its colour changed, the difference in the ſhade might not be obvious; beſides, ſome of our chemical diſcoveries are ſo recent, that time has been wanting to apply them to improve medicine extenſively; ſo that ſatisfactory comparative remarks on the colour of the venous blood in comſumptive patients are not to be expected ready made. We ſeldom perceive much more obvious phænomena than this probably can be, till we are prompted to look for them.

From Willis to Forthergill, and from Fothergill downwards, ſcarce any real obſervation occurs upon the ſtate of the blood. One tells us that it is polluted, another that it is contaminated, a third that it is acrid, a fourth that it is putreſcent *, without ever recollecting

* Leſt it ſhould be ſuſpected that *putreſcent* is here a ſound ſignificant, it ſhould be obſerved that 'when 'we examine the blood drawn from patients in every 'period of conſumption, ſo far from any appearance 'of

recollecting that to employ terms expreſſive of phænomena, ſuch as the ſenſes may recognize and to reaſon upon ſuch phænomena alone, are indiſpenſable conditions in philoſophizing.

Finding ſuch little ſatisfaction in the writers whom it was in my power to conſult, I applied to a gentleman whoſe opportuni-

'of diſſolution in its contents, the reverſe is conſtantly 'found; a *thick buffy* ſize and *firm* craſſamentum. 'Nay, ſo inconſiſtent are the favourers of this doctrine' (i. e. of the putrefaction of the fluids and ſolids in certain diſeaſes) 'that the *ſize*, and the degree of coheſion 'in the blood, has always been eſteemed an indica-'tion that the operation ought to be repeated, and 'much blood has been unneceſſarily ſhed accordingly. 'Nor in its progreſs do we obſerve any ſymptoms 'ſimilar to thoſe found in diſeaſes uſually termed pu-'trid; *no petechiæ, vibices, ſordes about the teeth, or blood 'iſſuing from the gums, and other parts of the body.*' Reid, p. 71—2. Dr. —— of Edinburgh, whoſe experience in blood-letting is immenſe, uſed to teach that in phthiſis there is an unconquerable morbid tendency in the ſyſtem to generate blood.—The firm craſſamentum, according to Dr. Gritanner, is a ſure indication of exceſs of oxygene.—Little diſpoſition to coagulate in ſcorbutic blood—the ſame in the *blue boy's* blood: a ſtriking coincidence!—oppoſite ſtate in phthiſis not leſs ſtriking.

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ties of inſpecting the blood of phthiſical patien have been ample, and whoſe fine experiments on frigorific mixtures atteſt his talents for obſervation, and will diſpoſe the public to confide in his accuracy. I aſked him ſimply, without any previous communication, whether he had noticed any particular appearance in the blood of phthiſical patients. 'Yes,' he replied, 'and that 'ſo conſtantly, that I believe I could generally diſtinguiſh blood taken from ſuch 'patients, eſpecially where the diſorder is 'confirmed.'—On the following written anſwer, he obſerved that colours are not eaſily deſcribed, and, perhaps, his terms might not be the beſt choſen: *florid* and *purple* ſeem indeed not well to agree.

DEAR SIR,

IN anſwer to your queſtion whether I recollect to have obſerved any particular appearance in the blood from phthiſical patients, I can inform you that I have always been as it were involuntarily, and without any particular deſign of attending

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to it, ſtruck with its thin conſiſtence, and its florid purpliſh colour.—Upon ſince putting the ſame queſtion to a foreign gentleman, who has paid particular attentionto its appearance, and even made experiments upon it, he replied, that it differed from other blood in being thinner, more florid, and having a purpliſh hue.

&c. &c.

Oxford,
July 25*th*, 1792.

RICHARD WALKER.

This foreign gentleman happened to viſit the Radcliffe Infirmary ſoon after my converſation with Mr. Walker. He has deduced ſome peculiar opinions on the nature of phthiſis from his reſearches on the blood taken from perſons labouring under the diſeaſe: theſe opinions, I underſtand, are quite different from that which I have propoſed, and indeed utterly foreign to it.

It is well known that the ſymptoms of phthiſis have been greatly aggravated in ſome patients who have been made to reſpire

ſpire oxygene air. Mr. Fourcroy deſcribes the reſult of the trial of oxygene air upon twenty patients, of whom he ſaw eleven himſelf. After a few flattering appearances, which inſpired them with very ſanguine hopes, they were all ſenſibly the worſe for this treatment, and as ſenſibly relieved by abandoning it. ‘Even amid their ſelf-congra‘ tulations,’ ſays he, ‘ ſeveral ſigns admo‘ niſhed the attentive phyſician that their ‘ hopes were ill founded. The ſkin was ‘ dry and hot; the face took fire and be‘ came of a more florid red, *s’allumoit et ſe* ‘ *coloroit d’un rouge plus vif qu’il n’etoit au*‘ *paravant*.’ This heightening of the colour by the inſpiration of oxygene air depoſes ſtrongly in favour of the opinion I am maintaining. Since the complexion, already more florid than natural, is heightened by the addition of oxygene, may we not conclude that the firſt gradation is alſo owing to an exceſs of oxygene. ‘ The ſymp‘ toms’ Mr. Fourcroy goes on to inform us, in a fortnight or three weeks after the firſt ſeemingly favourable effect of the oxygene

air

air ' became all at once more ſevere; the ' change was indicated by a dry convulſive ' cough, ſpitting of blood, a ſenſation of ' burning heat and ſharp pain in the thorax, a ' fever almoſt acute and threatening to be- ' come inflammatory, by agitations in all ' the members, reſtleſſneſs, and thirſt. ' It was neceſſary to bleed, to give anti- ' phlogiſtic and ſedative remedies, and the ' patients ſhewed great unwillingneſs to in- ' ſpire the oxygene air. When theſe violent ' and alarming ſymptoms were allayed by ' proper treatment, the diſeaſe reſumed its ' ordinary form, and the fever appeared with ' its quotidian type; the expectoration be- ' come purulent again. In its 4th ſtage the ' diſeaſe made a quicker progreſs than uſual. ' This accelerated progreſs, the ſymptoms of ' inflammation, the uneaſineſs, the oppreſſion, ' the burning (*ardeur*) of the lungs, the ſtop- ' page of the expectoration, the acute hæ- ' moptyſis, all theſe phænomena were ma- ' nifeſtly owing to the uſe of oxygene air. ' They equally took place in eight patients ' who were not ſo far gone as the others;

' and

'and it was neceſſary to abandon this mode 'of treatment—the patients themſelves in- 'deed deſired that it might be abandoned *.'

In the appendix to one of Dr. Prieſtley's volumes on air, ſome caſes are mentioned of phthiſis, in which the patients were ſenſibly relieved by breathing common air largely mixed with carbonic acid air. Dr. Percival tells us that he has 'adminiſtered 'fixed air in a conſiderable number of caſes 'of the phthiſis pulmonalis, by directing 'his patients to inſpire the ſteams of an 'effervefcing mixture of chalk and vine- 'gar, or vinegar and potaſh. The hectic 'fever has been conſiderably abated, and 'the matter expectorated has become leſs 'offenſive and better digeſted. I have not,' he adds, 'yet been ſo fortunate as to effect 'a cure. One phthiſical patient has, by 'a ſimilar courſe under Dr. Withering's 'care, entirely recovered, another was ren- 'dered much better, and a third, whoſe

* Annales de Chimie, iv.85.

'case seemed to be truly deplorable, seemed 'to be kept alive by it more than two 'months *.'

We cannot be surprised that these experiments should not have been attended with greater success, if we consider that those who made them could not at that early period be enlightened by the grateful dawn of a probable theory; that having no well-defined end in view, they could not vary their means with sufficient intelligence; and that, where the apparatus was so awkward, sufficient perseverance could not well be expected. If our object be to lower the standard of the atmosphere, carbonic acid air will not probably be chosen for this purpose. Should it be objected, that the abstraction of the oxygene was not continued long enough for the effect to be produced in this way, it may be replied, that in Mr. Fourcroy's experiments the application of oxygene was not probably continued much longer.

* Priestley, I. 301.

Some ſmall probability ariſes in favour of this theory, from the inconſiderable number of ſailors who die of phthiſis, unleſs the common books, as thoſe of Lind, Rouppe, Blane, &c. from which alone I draw my information, have impreſſed me with wrong notions on this point. Seafaring people are particularly expoſed to wet and cold, the exciting cauſes of phthiſis; a large proportion of them are, I ſuppoſe, of ſuch an age as, according to the common eſtimate, is moſt liable to be attacked by conſumption, and yet they ſeem to be even peculiarly exempt from this diſeaſe. In the ten years' regiſter kept by Mr. Gorſuch, at Shrewſbury, upwards of one fourth of the deaths appear under the title, conſumption. Though the bills of mortality are inaccurate in their denominations, yet their authority is ſufficient to prove, that vaſt havoc is made by conſumptions in London From the accounts and liſts of the ſick and dead on ſhip-board, conſidered with the neceſſary attention to the great difference of circumſtances, I ſhould not conceive ſo formidable an idea of of the ravages committed among ſeafaring perſons by this diſorder; before any ſafe judgment can be formed, however, it would be neceſſary to have more preciſe data than I have met with, and eſpecially to aſcertain how far it is common for perſons ſeized with conſumptions at ſea to die on ſhore.

Scorbutic perſons ought not, according to this theory, to be liable to phthiſis, nor phthiſical to ſcurvy; at leaſt it ſhould ſeem, that as one of theſe diſeaſes comes on, the other ſhould retire. It does not appear to me, that we have well-aſcertained facts enough here to afford a teſt of the truth or falſehood of the forgoing reaſoning. Dr. Lind, indeed, ſays, that 'perſons very 'much emaciated with the *flux* or *conſumption* are ſeldom or never ſeized with the 'ſcurvy *.' I ſuppoſe ſo careful an obſerver would uſe terms accurately, and more confidence may, perhaps, be placed in his experience in the ſcurvy, than in that of all

* p. 508.

other writers put together. But in Mr. Ives's Journal it is ſaid, that 'five or ſix ſcor-'butic men, who had coughs, are now in 'deep conſumptions *.' Again he ſays, 'ulcerated lungs is a common conſequence 'of the ſcurvy †.' Dyſpnœa, tightneſs and pain of the breaſt with coughing, are among the ordinary ſymptoms of the ſcurvy; nor will this appear extraordinary to a perſon who conſiders that neither the lungs, nor the left auricle and ventricle of the heart, perform their functions properly. It is therefore very poſſible to confound the pulmonic ſymptoms of phthiſis with thoſe of ſcurvy, though they ſeem perfectly diſtinguiſhable, by the abſence of hectic fever in the latter caſe, and by their yielding to vegetables. I have already endeavoured to ſhew, that very frequently the immediate cauſe of death in the ſcurvy is the diſeaſed ſtate of the contents of the thorax; and if all circumſtances be impartially conſidered, we ſhall conclude that Mr.

* p. 92.

† p. 107. May there not be *ſcorbutic* ulcers of the lungs?

Ives's

Ives's account is not explicit enough to satisfy us, what effect phthisis produces upon the scurvy; whether the two disorders go on together, and whether the ulcerations of the lungs are such as take place in phthisis, accompanied with tubercles, &c. It would be curious to know how the slow and feeble pulse, natural to scurvy *, is modified by those causes which in phthisis render the pulse so frequent and so hard. Lind relates, from his own experience, that in the *Salisbury* man of war, in May 1747, 'when there prevailed several inflammatory 'disorders, particularly peripneumonic fe- 'vers, or inflammations of the lungs, all who 'were recovering from them became highly 'scorbutic †.' This observation is so far from forming a difficulty in the way of any part of the preceding theory, that it remarkably confirms what has been said of scurvy. Those who die of pneumonia die, I believe, of suffocation; the countenance appears bloated, discoloured, and li-

* Lind, p. 108. † p. 74.

vid;

vid: even when the expectoration, upon which ſo much depends, goes on favourably, we may reaſonably ſuppoſe that leſs oxygene will be received through the lungs than in perfect, health during that debility of the thoracic organs which ſucceeds the ſtate of inflammation. Hence the ſcurvy will eaſily come on, when its exciting cauſes are at hand.

I do not know whether the emaciation preceding and accompanying phthiſis will be allowed to afford me any additional preſumption. Conſumptions have been ſaid to originate from the abuſe of vinegar and ſour fruits; but here I believe there is an inaccuracy in terms. In ſome inſtances of the diſorder ariſing from this cauſe, I have obſerved it to affect the abdomen, and not the thorax. The ſtomach and bowels are probably burned by the long continued application of even weak acids; and hence atrophy enſues, as the chylopoietic viſcera become unfit for the office of nutrition.

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The beneficial effect of the mineral acids in allaying the hectic symptoms for a short time would be a contrary probability, if acids have not some immediate action, independent of their composition. The use of nitre in incipient phthisis, if nitre, which I doubt, be really beneficial, would also form another objection.—It would therefore be rash to place much confidence on so incomplete a theory, however strongly it may seem to be favoured by some of the principal phænomena. If I might even take it for granted that excess of oxgene is a well-ascertained circumstance in phthisis, it would still remain to be determined, before the investigation could lead to any thing useful, what rank it holds among the other deviations from a state of health observable in this disease.

Here two suppositions occur: 1. The phthisical inflammation may so alter the structure of the lungs, as to cause them to transmit a more than ordinary portion of oxygene to the blood; or, 2. Some unknown

known caufe having enabled them to tranf-mit, or the blood itfelf to attract, more oxy-gene, an inflammation of the lungs might enfue *.

The following obfervations of Mr. La-voifier may perhaps affift the reader's re-flections, as well as illuftrate feveral points of the preceding difquifition. That great philofopher had confined a Guinea pig for an hour and quarter in 248 cubic inches of

* During fthenic inflammation, does not a too rapid combination of oxygene take place, of a kind fimilar to the fecondary combination defcribed above, in confe-quence of which blood changes from florid to black? May not the heat attending inflammation depend upon this fecondary combination? Does not the livid colour fucceeding violent inflammations countenance this fup-pofition? or is that colour folely owing to ftagnant ex-travafated blood, in which the oxygene undergoes this fecondary combination? We muft feek the explana-tion of the mechanifm of fuch changes in the prin-ciples of chemiftry; and the knowledge of this me-chanifm will not fail to be ufeful. If, for inftance, the above hypothefis were true, it would not be difficult to draw from it fome practical inferences refpecting the prevention of gangrene.

oxygene air. 'Towards the cloſe of the 'experiment,' ſays he, 'the animal ap-'peared to ſuffer conſiderably. Neverthe-'leſs, only a very ſmall portion of the air 'was vitiated, i. e. converted into fixed air; 'there remained, after the animal was re-'moved, much more vital air than is ne-'ceſſary to conſtitute a ſalubrious atmo-'ſphere; this circumſtance,' viz. the diſtreſs and death of animals confined in oxygene air, long before it had become unfit for reſpiration, 'had been noticed by Dr. 'Prieſtley. Having occaſion to repeat ſome 'of his experiments, I choſe Guinea pigs 'for the purpoſe: the air in which they 'were confined, was nearly pure; it did not 'contain above five or ſix parts in an hun-'dred of azotic air. Although the animals 'lived much longer in this than they would 'have done in an equal volume of atmoſ-'pherical air, they nevertheleſs all died 'long before it was completely vitiated; 'and other animals introduced into the air 'in which they had died did not appear to 'ſuffer, at leaſt for ſome time. It is not

T 'therefore

'therefore for want of air fit for reſpira-'tion that animals die in vital air, but from 'ſome noxious effect of that air.

'Dr. Bucquet aſſiſted at ſome of my ex-'periments; and we opened the animals 'that had died in vital air. In every in-'ſtance death ſeemed to have been occa-'ſioned by an ardent fever, and an inflam-'matory diſeaſe. The fleſh was of a *very* '*red* colour; the heart livid, and turgid with 'blood, eſpecially the right auricle and ven-'tricle; the lungs were very flaccid, but '*very red*, even externally; they were alſo 'turgid with blood.' (*Mem. de la Societé R. de Medicine*, t. V. p. 575—576.)

The unuſual animal heat, which muſt have been generated in theſe experiments, the ſtimulant power, which, independently of the heat, oxygene confers upon the blood, that irritability which it communicates to the ſolids; all theſe cauſes might eaſily produce the inflammation obſerved by Mr. Lavoiſier. Now may not the flower and differently

differently modified inflammation of the lungs, in phthiſis, originate from a ſmaller exceſs of oxygene thrown into the ſyſtem in a more gradual manner?

According to the former of the two ſuppoſitions ſtated above, we might hope ſometimes to ſucceed in curing the diſeaſe by withholding oxygene, and giving the pulmonary ulcers an opportunity to heal; according to the ſecond, the diſeaſe would be ſtill more in our power; by removing the cauſe that produces and continues it, we might, with greater certainty, expect the inflammation to ſubſide.

Of theſe hypotheſes, I think it ſome recommendation that they lead to a project totally different from the nugatory modes of practice heretofore employed. The treatment they ſuggeſt is ſo obvious, that it is ſcarce neceſſary to add a ſyllable on the ſubject. Fruits, herbs, milk, &c. with all their cooling and all their occult qualities beſides, have never, I ſuppoſe, effected a cure of phthiſis; nor am I acquainted with any reaſon capable of ſatisfying a perſon at all ſolicitous in forming his opinions to diſcriminate truth from falſehood, that they have ever contributed towards a cure. While the diſeaſe is forming, indeed, at which time the diſorder ſeems to be highly inflammatory, an oppoſite diet may accelerate its progreſs. But there will, probably, be little difficulty in prevailing upon men of reflection to avoid both a vegetable and a ſtimulating diet; and to put their phthiſical patients upon ſuch a diet as, according to the idea of that diſeaſe already ſo frequently repeated, ſhall tend to produce the ſcurvy. Not only ſalted meat, but an oily diet, may be tried. It will not however, I imagine, avail us much ſolely to cut off the ſupply of oxygene by the ſtomach. The lungs themſelves being diſeaſed, and alſo being the moſt copious ſource of oxygene, it would be moſt advantageous to ſupply them with an air ſuited to our purpoſe; ſuch an air ſhould be mixed either with an additional quantity of azotic or with hydrogene air, which ſeems to have no irritating quality, and has been found to have the

power

power of darkening the colour of the blood. We cannot expect benefit from the air of a crowded room, ſince its temperature may counteract the effect of its diminiſhed proportion of oxygene. It is poſſible, but by no means certain, that the ſteams abounding in ſuch a room, which have been complimented with the title of *putrid*, may be injurious to conſumptive perſons. Till ſome means of lowering the ſtandard of atmoſpheric air, without adding to it any thing hurtful, ſhall be contrived, we may remove phthiſical patients out of thoſe airy ſpacious apartments which of late have been thought ſalutary in all diſeaſes indiſcriminately. They may at leaſt ſleep in confined rooms; and the more confined the better, provided a cool temperature be maintained.

Here it may be aſked, with reference to this practice, whether conſumptions deſtroy a larger proportion of the inhabitants of the town or the country. I ſhall have occaſion to ſay a few words on this ſubject below.

below. In the mean time, we can ſcarce expect any effect from the ſtate of the atmoſphere, ſince it appears to contain an equal quantity of oxygene air in the moſt populous city and the cloſeſt weather *.

It has ſometimes been ſuppoſed that hœmoptoe and phthiſis have been produced by quickſilver †; and this fluid metal has been

* According to the experiments of Mr. Scheele and Caveudiſh.

† In the ſcurvy, preparations of quickſilver in extreme ſmall quantity produce a copious and dangerous ſalivation, almoſt always attended with bloody ſtools. (*Lind*, *p*. 111.) A tendency to ſalivation is frequently obſerved in ſcorbutic perſons, independent of quickſilver. The chemical condition of the ſyſtem, I ſuppoſe, is the cauſe of the firſt mentioned appearance. But in what this conſiſts, I cannot form any ſatisfactory conjecture at preſent. I think it alſo not preſumptuous to expect from chemiſtry the explanation of the peculiarity, which appears in ſome perſons, whoſe ſkin is no ſooner touched with quickſilver ointment than it is felt in the ſalivary glands. Moſt idioſyncraſies depend probably upon ſomething peculiar in the chemical compoſition of the ſyſtem. Of ſuch effects there muſt be determinate cauſes; and I ſee none ſo likely

been imagined to increaſe the momentum of the blood ſo much as to break through the looſe and tender veſſels of the lungs. This is a very clumſy account of the beginning of phthiſis, and ſuch groſs mechanical ideas are doubtleſs inapplicable to the motions of the living ſyſtem. Quickſilver is taken in oxygenated, and thrown out reduced; and the properties of oxygene, and its aſcertained connection with the functions of animals, ſeem to afford a far more appropriate explanation of the phænomenon.

The fact, if it might be explained in this way, would corroborate a ſuppoſition which is ſuggeſted by a common appearance; the ſuppoſition is, that the hyper-oxygenated ſtate of the ſyſtem precedes thoſe ſymptoms which characterize phthiſis. I am inclined to believe that the fatal and peculiar inflammation of the lungs, whether indicated by acute pains in the cheſt, ſpitting of pus, &c.

likely as this: I do not, therefore, wonder that theſe phænomena have hitherto appeared ſo unaccountable; for we have totally wanted data to explain them.

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is posterior to the accumulation of oxygene. In persons only threatened with consumption, and before any formidable signs of disease in the thorax occur, we often observe hæmorrhages from the nose (in which I have thought the blood preternaturally florid) as well as a remarkable brightness of the complexion. Thus Mr. Portal, the celebrated anatomist, and one of the latest observers, speaking of persons, *menacés de tomber dans la phthisie*, mentions the *rougeur souvent habituelle de leur visage**: this indeed is a very common place observation.

I add a general consideration, which may possibly induce the physiologist to weigh the pretensions of the foregoing hypothesis with deliberation. The functions of the lungs seem to have been determined by modern experiments with great precision; they are destined to transmit oxygene to the blood, while at the same time a quantity of carbone and hydrogene passes off in a contrary direction, and uniting with the oxy-

* Esprit des Journeaux, Mars, 1792, p. 362—2.

gene of the atmofphere, forms carbonic acid and water. Hence one might conjecture that the lungs will be apt to deviate from an healthy ftate in two oppofite ways; in one of their morbid conditions, the combinations that take place within the thorax will be impeded; in another, they *may* be carried on to too great an extent; and then, the lungs being the principal focus of animal heat, they might be injured by being conftantly expofed to too high a temperature, or by having too much oxygene offered to the attractive power of their own fubftance.

Here I forefee, without dreading, a fpecious objection. 'Since the lungs are dimi-'nifhed during the progrefs of phthifis, is it 'likely that they fhould, with a narrower 'area, carry on, to too great an extent, the 'combinations to which they are deftined? 'Is it not alfo to be expected that the inflam-'mation fhould render them lefs permeable?' It might be replied, that the great mifchief feems to be done before they are either much thickened by inflammation, or wafted by abforption. Befides, it is common enough for oppofite conditions to exift either throughout the whole fyftem, or in a particular organ, at different periods of a difeafe, as excefs of action is followed by debility. I however think it probable that inftances have occurred, where the lofs of a confiderable part of the fubftance of the lungs has checked the progrefs of the difeafe. I knew a perfon who died, after a tedious ftruggle, of phthifis. The ribs on one fide were preffed quite inwards, in confequence, I fuppofe, of the deftruction of the correfponding lobe of the lungs. From this time, the difeafe, which had at firft proceeded at its ordinary rate, went on wonderfully flow; and it was not till feveral years after the depreffion of the ribs that the patient died, without ever having had a diftinct and long continued intermiffion. However this may be, in the generality of cafes actual obfervation fhews the objection to be groundlefs. In the various fhades of livid colour, which are produced by a total

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or partial exclufion of that portion of oxygene, which enters the blood by way of the lungs, we have nearly as good a criterion of any confiderable deficiency of this principle as we can defire. Now I believe it feldom or never happens that a livid fuffufion overfpreads the countenance of a phthifical patient: *les rougeurs*, fays Mr. Fortal, *augmentent prefque jufqu' au dernier moment;* and in proportion as the dark-red afthmatic flufh is a ftranger to this difeafe, we may infer that even the phthifical dyfpnœa does not prevent a full fupply of oxygene.

'The more you reflect, the more you will be convinced that nothing would fo much contribute to refcue the art of medicine from its prefent helplefs condition, as the difcovery of the means of regulating the conftitution of the atmofphere. It would be no lefs defirable to have a convenient method of reducing the oxygene to 18 or 20 in 100, than of increafing it in any proportion. The influence of the air we breathe is as wide as the diffufion of the

 blood.

blood. The minuteſt portions of the organs of motion, ſenſe, and thought muſt be affected by any conſiderable change in this fluid. Whether it be that the brain muſt be waſhed by ſtreams of arterial blood, or that the action of every organ is a ſtimulus to the ſyſtem in general, and conſequently to every other organ in particular, it is certain, that when the acceſs of oxygene is cut off from the lungs, the functions of the brain ceaſe: perhaps there may be a mixture of azotic and oxygene airs more favourable to the intellectual faculties that that which is found in the atmoſphere; and hence chemiſtry be enabled to exalt the powers of future poets and philoſophers. That diſeaſes of excitement on the one hand, and debility on the other might be cured almoſt ſolely by a proper air one can hardly doubt, as well as ſeveral diſorders at preſent highly dangerous or deſperate, which one cannot, upon the faith of any obvious phænomena, refer to either head. The materia medica might, therefore, undergo a ſtill greater reduction, than it has lately undergone in conſequence

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of the purification of medicine from its groffer abfurdities; and hence the treatment of difeafes be at once rendered infinitely more pleafant and more efficacious.

The extreme frequency of confumptions in Great Britain feems to have been juftly afcribed to the variablenefs of the climate. A few reflections will, perhaps, render it probable that the caufe is adequate to the effect, at leaft that it is peculiarly calculated to excite pulmonary inflammations, with the concurrence of other circumftances. Climates of equable warmth appear to be moft favourable to health, and much the moft congenial to the human race. Next to thefe may be placed thofe climates which have fixed feafons; the moft deftructive are the variable. In England we feldom enjoy any continuance of fettled fine weather, except towards the clofe of fummer and the beginning of autumn, and even then we are frequently baulked in our expectations of fine weather. The fudden changes that take place during three fourths of our year

may

may be regarded as no leſs prejudicial to the health, than diſagreeable to the feelings; and our terrors of *catching cold*, which have frequently appeared ridiculous to foreigners, are really better founded than we ourſelves are apt, moſt of us, to apprehend; colds, in their conſequences, proving fatal every year to multitudes both of the young and the old; to the former principally by giving riſe to conſumptions, to the latter by producing pulmonary diſeaſes of a different character.

We cannot hope entirely to eſcape the unpleaſant ſenſations, or to ward off the fatal effects, occaſioned by this caprice of our climate. But by underſtanding how colds are caught, we may be induced to take certain ſimple, but uſeful precautions. One may with the greater propriety embrace an opportuniting of diſſeminating this ſort of information, as the manner in which colds or inflammatory cattarrhs is taken, though now in my opinion perfectly aſcertained, is far from being generally underſtood by the members

members of the medical profeſſion; and if any perſon, not belonging to that profeſſion, ſhould ſuſpect this to be a wanton paradoxical aſſertion, he may find in the caſe of opium, and of the cool treatment of ſmall pox, &c. inſtances equally ſtriking, where one generation of pathologiſts paſſed away after another, without being able, in the caſe of opium *, to perceive the plaineſt appearances, or, in that of ſmall-pox, to draw the ſimpleſt concluſion. So ſervilely imitative an animal is man! ſo loath to employ his own powers of perception and thought!

* It is curious to ſee what pains medical writers have taken to imagine hypotheſes, either out of mere complaiſance to the term, *narcotic*, or becauſe opium is a drug in Chriſtendom, and wine an article of diet, rather than ſuffer themſelves to ſee that opium makes a man merry or drunk, then lays him aſleep, and afterwards cauſes him to awake with an head-ache, in the ſame manner as wine. One cannot compare Haller's clear and ſatisfactory parallel of wine and opium, publiſhed in 1769 (*El. Phyſiolog. t. V. p.* 610—11) with Cullen's perplexed and hypothetical doctrine of opium, and his whole article *ſedantia*, publiſhed in 1789, (*Mat. Medica, t. II.* 217, *& ſeq.*) without a ſenſe of humiliation.

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When any part of the body has been expofed to cold, it is liable to be much more affected by heat and other ftimuli than before the expofure. Of this the method of treating frozen limbs in cold countries affords a beautiful and decifive proof. Were a frozen limb to be brought before a fire, or immerfed in warm water, a violent inflammation would come on, and fpeedily terminate in mortification. They therefore take fnow to rub the parts benumbed with cold, and very gradually expofe them to a warm temperature. The glow, after coming out of the warm bath, is entirely owing to the lower temperature to which the body has immediately before been expofed, or, what amounts to the fame thing, the power of water to conduct heat away from the body fafter than air. The pungent pain felt upon holding an hand much chilled to the fire is another exemplification of the fame principle, which feems to be one of the moft general laws of animal nature. In like manner the ceffation of action and thought during ſleep accumulates a power of thinking and acting with more energy than before we fell aſleep. Even Dr. Cullen, whom the prejudices of old age and the pride of celebrity conſpired to hinder from receiving this doctrine in its full extent, in his laſt work expreſſes himſelf upon the article of ſleep with a preciſion that is not always found in the theoretical part of his writings; 'a ſtate of 'ſleep,' ſays he, 'ſubſiſting for ſome time, 'induces a ſtate of the ſyſtem *more ready* '*to be affected by ſtimuli of all kinds* *.' The latter part of the ſentence is a very accurate and luminous interpretation of the common expreſſion, *that we are refreſhed by ſleep*.

* *Materia Medica, II.* 228. In his little book on phyſiology ſome ingenious hints towards this doctrine will be found: Brown doubtleſs profited by theſe hints, and, I fear, without due acknowledgment; the old Profeſſor, on the contrary, who had gone into other theories, which he outlived, could not bear to think that a man, who had been almoſt his ſervant, ſhould have matured his ideas into a ſyſtem highly ingenious and partly juſt.

Now after the application of cold, which, according to circumſtances, produces a greater or ſmaller diminution of the actions of the living ſyſtem, and at length ſleep itſelf, there may be an infinite number of gradations between a fatal inflammation and a tranſitory glow, and this according as the previous cold and the ſubſequent heat have varied in intenſity; but whatever be the degree, the effect depends on the ſame principle. By reſpiring a cold atmoſphere the ſame thing happens to the noſtrils, fauces, lungs, as to the external ſurface of the body upon going into a cold bath; and if we paſs ſuddenly from ſuch an atmoſphere into a warm room, what happens to the ſkin will in ſome degree happen to the membrane lining theſe cavities; a glow or inflammation will enſue, according to the difference between the two temperatures and the length of time paſſed in the cold. When the application of cold or moiſture to a ſuperficial part only is ſucceeded by an inflammation of the reſpiratory cavities, the conſent of the whole

ſyſtem

ſyſtem eaſily explains this remote local affection. The cauſe of diſeaſe pervades at once and feels as it were, or ſearches the whole body, but affects only in a degree to draw our notice to the organ which fr m habit or ſtructure is moſt tender. Should any other part, from previous circumſtances, have been rendered more ſenſible to its influence, we ſhall in conſequence have a ſore throat, a diarrhœa, or the rheumatiſm, in place of a catarrh. Children are ſo ſuſceptible of inflammations that a great part of the mortality among them is, as far as I have obſerved and can judge, to be aſcribed to the ignorance of mothers and nurſes of the power which even a moderate change of temperature, if ſuddenly made, has to affect their tender and irritable frame. Whenever accurate regiſters of the mortality of the human ſpecies, in climates equably warm, ſhall be kept, I expect that not half ſo many infants will be found to die as in Britain. Hence, in part, the populouſneſs of ſuch countries: thoſe gardens of the earth being equally calculated to rear and ſupport inhabitants.

In catarrh therefore, whatever be the degree of inflammation, from the ſlighteſt affection of the noſtrils or cheſt to the moſt acute pleuriſy, it never takes place, unleſs we paſs too ſuddenly into a comparatively warm atmoſphere, or apply a ſtimulus equivalent to heat. The feveriſhneſs, the internal glow, the dryneſs of the noſtrils, the *huſkineſs* of the bronchiæ, are never felt while we remain in the cold; they ſpeedily come on after entering a warm room. Perſons who can recollect their paſt, or will attend to their future ſenſations, will eaſily be convinced of the truth of this ſimple and ſalutary theory. Formerly, when I firſt heard it, I muſt own that I durſt not believe it on account of its ſimplicity *. It was

* As far as I can judge from recollection, the following latent hypotheſis was a great cauſe of doubt. Anatomy ſeemed to demonſtrate a wonderfully complicated ſtructure in the human body. It was obvious to imagine

was not till having been made more attentive to facts by a desire to determine the merits of the different theories of catarrh, that I was irrisistibly convinced by my own personal experience of the justness of that shortly stated above, and which indeed requires but very few words for its exposition. Frequently after riding for hours in the rain, especially during summer, I have felt a glow infinitely more vivid than in coming out of the cold bath. This glow was owing partly to the temperature of the atmosphere, and partly to the slight exercise of changing cloaths. I have sometimes made other persons attentive to the progress of the phænomena, and nothing has appeared more evident than that during exposure to wet or

gine that the functions or use of this complicated structure must be complicated in proportion. This is a false conception: the living body is in reality infinitely more simple than it appears at first view, at least as to the actions and diseases of its parts; and the structure is but a constant repetition of nerves, blood-vessels, cellular substances, fibres, and fat. The anatomical knowledge of a surgeon, indeed, must be minute, but it is not abstruse; not much more so than that of geography. It is perfectly analogous to subterraneous geography.

cold,

cold, no tendency to inflammation is perceptible, but that ſubſequent heat, exerciſe in the dry, and ſtimulants, produce the glow or inflammation. By keeping quiet and cool for ſome time after being wet in ſummer, and by avoiding a ſudden tranſition into a warm temperature in cold weather, and by temperance in both caſes, thoſe inflammatory diſeaſes, for which cold only prepares the ſyſtem, may be eaſily avoided; and any perſon, by acting upon theſe principles, may have at pleaſure a ſlight or a violent catarrh, or no catarrh at all.

The popular treatment, therefore, of colds during their early ſtage is juſt as prejudicial, as the ancient hot regimen during the ſmall-pox. Warm or ſpirituous liquors, warm cloſe rooms, and a weight of bed-clothes cannot but aggravate the ſymptoms. The ſame may be ſaid of Mudge's inhaler, which though certainly ſerviceable in the aſthenic catarrh, or *catarrhus ſenilis*, I have oftener than once obſerved ſenſibly to heighten

heighten the inflammation at the commencement of a common cold *.

It

* *Catarrhum igitur e frigore eſſe, calore ſolvendum, graviſſimus error eſt. Contrà, frigus nunquam nocet, niſi ubi ejus actionem calor excipit.——Catarrhus æſtate toties incidens, ubi ſexcenties cauſa ejus a frigore ſupra repeti nequit, a calore poteſt; contagioſus nunquam, communis ſæpe; frigoris egens; non omnino frigori, calori protinus ſuccedens;——obſervationem condem firmant.*

Such is the doctrine of Brown, (Elem. Medicin. II. 42, 43) a *noted* author and teacher, as he is ſtiled by Cullen, (Mat. M. II. 235.) who was very deſirous to perſuade himſelf that the adverſary whom he notes, and againſt whom he proteſts, was contemptible. For my part, I conſider his doctrine of the effect of ſtimulating powers applied to accumulated excitability as the only ſpecimen of extenſive reaſoning in pathology, calculated to afford any ſatisfaction to a juſt thinker. He avoids thoſe unmeaning or vague terms, that had been before ſo much employed to ſhelter ignorance from their employers and from others; he appeals to phænomena of the living ſyſtem obvious to the ſenſes; and adopts ſuch principles of reaſoning that, if he has not always diſcovered the truth, he is ſeldom forſaken by the ſpirit of a philoſopher.

I beg leave to add—not for the ſake of propitiating medical orthodoxy, but of preventing miſapprehenſion—that in his ſyſtem of opinions and practice, as delivered either in his lectures or writings, I have always found full as much

It has been, I believe, unfortunate for the inhabitants of this country, that we are not subject

much to reject as to receive. His directions for the application of stimuli often seemed contrary to his principles; and, I believe, he led his followers into dangerous mistakes on this important point. I have nothing to say in defence of his personal conduct, which more than any thing else caused his doctrines to be condemned unheard, as if the grossness of a man's manners affected the conclusiveness of his arguments. If his imprudence, however, was highly blameable, and his arrogance intolerable, the liberal will allow something to the deep consciousness of neglected merit, and to the irritating sensations attendant upon declining health: and posterity may reproach an age, in which a man, possessed of powers so superior and so nobly exerted, was browbeaten, defamed, almost persecuted, and left to perish in extreme penury.

It is to be lamented that Dr. Brown's writings should be so little known to those who are secretly influenced by his opinions, which in one way or other have been so widely diffused as to affect almost the whole practice of medicine in Great Britain. His mistakes a dispassionate man would easily distinguish and avoid; but as he certainly did not err wilfully, I know not why they should be held up as criminal, or censured with greater asperity than the exploded theories of Boerhaave or Cullen.

From conversation with the author of the *Batonic* subject to such a continued severity of cold, as should oblige us regularly to fortify ourselves by warm cloathing. By linen, worn exclusively, we lose more in health than we gain in comfort; which comfort is, perhaps, after all, merely imaginary; for there is hardly an instance in which the skin does not soon reconcile itself to woollen, though there is no necessity for placing it next the skin, and cleanliness is just as much in the power of the wearers of woollen. The most simple method, as well as the

Garden, who is no less eminent as a physician than a poet, I find that he entertains similar sentiments concerning the Brunonian doctrine. Several of Dr. D.'s friends have indeed assured me, that he had discovered these principles many years ago, and reduced them to a regular system. Indications of such a system appear in some of the notes to his beautiful poem, but the complete treatise has never got beyond the circle of his friends; a rare example of modesty or indifference to fame! It is much to be wished that the anticipation may not finally deprive us of the treatise: some peculiar illustrations it must have, every one of which would enlighten the philosopher and guide the physician. The authority of the writer too would secure the doctrines a fair hearing, and his ample experience would inspire confidence in his reasonings.

moſt effectual, to avoid the influence of ſudden changes of atmoſpherical temperature is to wrap the body in ſubſtances that conduct heat ſlowly. Both for this reaſon, and becauſe it is ſo much leſs unpleaſant, when moiſt, than linen, flannel ſhould be worn at leaſt above linen during every ſeaſon in Great Britain; and thoſe who feel it neceſſary may double it during the winter, ſpring, and beginning of ſummer.

In children it is of the utmoſt conſequence to keep the body cool, but never to ſuffer it to be cold. Thus, without being enervated, they may eſcape the fatal conſequences of heat ſucceeding quickly to cold; for it is not true, as ſeems, in conſequence of an analogy more or leſs diſtinctly conceived, to have been frequently imagined, that cold hardens children as it hardens ſteel.

Perſons advanced in years and ſubject to the aſthenic catarrh, ſuffer from the immediate application of cold or moiſture, which

which is but cold in another form. The tranſition to a cold or damp air will immediately affect them: I have obſerved the ſymptoms to commence as ſoon as one breath of ſuch air was inhaled; and in ſome caſes they are very quickly freed from dyſpnœa, wheezing, defluxion, &c. upon removing into a warm dry air; inſomuch that there is a ſtate of the diſeaſe when a perſon may take and loſe this ſort of cold ſeveral times a day. I am afraid dreſs alone will never prove ſo effectual a preſervative againſt this as againſt the other kind of catarrh. If ſome portable apparatus for warming the air before its admiſſion into the lungs could be contrived, this perhaps would ward off thoſe pulmonic attacks, which though only dangerous at a certain age, or in a ſtate of conſiderable violence, are at all times diſtreſſing enough to be numbered among the innumerable evils of a moiſt and variable climate.

A very recent writer, by no means deficient in acuteneſs, queſtions, or rather ſhews an inclination to queſtion, the baneful effects ariſing from the variableneſs of our climate. ‘ There is, perhaps,’ he obſerves, ‘ no coun-‘ try where coughs are ſo frequent as in ‘ Great Britain, which is generally imputed ‘ to the exceſſive variableneſs of the cli-‘ mate. But this cauſe ſeems to have no ‘ ill effect upon the health of wild animals, ‘ who are expoſed to the ſame uncertain ‘ weather.’—This variableneſs ‘ does not ‘ prevent many vegetables and animals from ‘ arriving at their utmoſt perfection.

‘ The blooming complexion of our pea-‘ ſantry—the permanence of their good ‘ looks, and their ſtrength and activity ‘ ſtrongly ſhew that the climate is not un-‘ wholeſome to thoſe properly educated. ‘ Strangers, however, often ſuffer by the ſud-‘ den tranſitions from heat to cold, and from ‘ dry to moiſt, which are ſo frequent. And ‘ thoſe natives ſuffer likewiſe who are de-‘ licately brought up, and who, from being ‘ generally confined in warm apartments, ‘ ſeldom feel the natural temperature of the ‘ air,

'air, and acquire in this northern region 'conſtitutions adapted to Italy.—They ac-'quire' (from change of weather) 'catarrhs, 'pleuriſies, conſumptions, ſore throats, fe-'vers, and other diſeaſes.

'But theſe ailments ought rather to be 'imputed to a delicate conſtitution, acquired 'by an improper mode of life, than to the 'climate.'—He goes on to ſay, that an infant born healthy, and hardily brought up, would be nearly as little liable to injury by the variableneſs of the weather as a young fox *.

Several reflections connected with the preceding obſervations are ſuggeſted by theſe paſſages. 1. If the wild and unhouſed animals are leſs ſubject to pulmonic diſeaſes, it is becauſe thoſe diſeaſes are rarely produced by natural heat; or by any ſtimuli, to which ſuch animals are expoſed. There cannot well be a more deciſive proof, than this general fact, of the truth of the theory. Upon

* Moore's Eſſay on the Materia Medica, 1792, p. 280, &c.

this

this principle we may account for an appearance that has often ſtruck the pathologiſt. The catalogue of the diſeaſes of wild animals is extremely ſhort. Domeſtic animals being leſs ſubject to the action of artificial heat and ſtimuli than man, but more ſo than wild animals, the effect is found in this inſtance alſo to be in proportion to the cauſe. Thus, as the author juſt quoted well obſerves, except a few domeſtic animals, ſuch as are ſtabled in warm buildings, or tended with human care, others are little liable to catarrh *. The moſt tender ſeem liable to few, except diſorders ariſing from the exceſs, or defect, or unwholeſome qualities of their food, and theſe are chiefly diſorders of the alimentary canal; by far the greater number would ſurvive to a good old age, if left to themſelves, and not, like half the human ſpecies in our climate, ſink back into the tomb, as ſoon almoſt as they have catched a few glimpſes of the light of heaven. 2. If delicate perſons are ſubject

* *Coughing*, the moſt equivocal of ſymptoms, may ariſe from a diſeaſed liver, &c. &c.

to

to diseases and dangers in England to which they would not be subject in Italy, it is evident that the difference between the climates of England and Italy is the cause of these diseases and dangers. 3. I am much afraid that the greatest mortality of children is among those hardily brought up. It is, I conceive, impossible for a man of humanity, or even of common reflection, to read the following passage without shuddering at the extreme wretchedness, and the wide extent of that wretchedness, which it announces. 'Poverty, though it does not prevent the generation, is extremely unfavourable to the rearing of children. The tender plant is produced, but in so cold a soil and so severe a climate, soon withers and dies. It is not uncommon, I have been frequently told, in the Highlands of Scotland, FOR A MOTHER WHO HAS BORNE TWENTY CHILDREN NOT TO HAVE TWO ALIVE*.' In England the children of the poor are swept away by hundreds and by thousands;

* Smith's Wealth of Nations, I. 120.

and

and the axiom of medicine, *penuria morborum causa*, is, I believe, even more ſtrikingly verified in the caſe of poor children than of their parents. But I ſuppoſe from what follows, that the author means by *hardily brought up*, ſuch as are not enervated. 4. I am much afraid, however, that the peaſantry, after weathering the early inclemencies of their ſtation, enjoy no ſuch advantage of freedom from pulmonic complaints as he ſeems to imagine. To mention one or two probabilities to the contrary: an inconſiderable city, and a thinly inhabited country, yield to the Oxford infirmary but a ſcanty ſupply of patients; yet I have been told, that for a great number of years paſt, there have been almoſt always among the number ſome ill of phthiſis, or threatened by that diſeaſe. Here it may be remarked, that neither confinement in cloſe rooms, unwholeſome fumes, or an unfavourable poſture, contribute any more than an effeminate education, to the production of conſumptions. Again, among the peaſantry of Warwickſhire and Staffordſhire, I underſtand that conſumptions are extremely frequent; not leſs ſo than among the Birmingham manufacturers. And who has not often met with phthiſis in the moſt airy ſituations?

Among the richer claſs this fatal diſeaſe would, perhaps, be leſs common, did they not by a ſtrange infatuation take the moſt effectual ſteps to contract it. If a greater proportion of females fall victims to it, is it not becauſe, loſing ſight more than men of its primary purpoſe, they regulate their dreſs ſolely by fantaſtic ideas of elegance? If happily our regret ſhould recal the age of chivalry, to break the ſpell of Faſhion would be an atchievement worthy the moſt gallant of our future knights. Common ſenſe has always failed in the adventure; and our ladies, alas! are ſtill compelled, whenever the Enchantreſs waves her wand, to expoſe themſelves, half undreſſed, to the fogs and froſts of our iſland.

TWO MEMOIRS

TRANSLATED FROM THE FRENCH

OF

DR. GIRTANNER*.

MEMOIR I.

THE irritable or contractile, improperly called the muſcular fibre, when ſeparated from the animal or the plant, preſerves its irritability for ſome time, and continues to contract upon the application of any ſtimulus. It retains alſo this property when it is cut into pieces, as is obſervable on dividing the proboſcis of a butterfly, or the ſtamina of plants. Each piece continues to contract, which proves that the ſmalleſt portion of the fibre poſſeſſes its

* N. B. Part of the firſt memoir is omitted.

own peculiar irritability, independent of the other portions. The fluids in animals and vegetables are endued alſo with this property as well as the ſolids. Their irritability conſiſts in coagulability, which coagulability in fluids is ſubject to the ſame laws as the irritability of the fibre. This is a new diſcovery; and it forms the baſis of many important truths. The degree of irritability of both ſolids and fluids is conſtantly changing; it varies according to the age and habit of the ſame animal and the ſame plant, and according to the ſex, organization, and ſize of the different individuals. It is encreaſed by the abſence of the common and habitual ſtimuli, and is weakened by the too frequent application of the ſame ſtimulus, or by the application of a ſtimulus too powerful. We may diſtinguiſh three different ſtates of the irritable fibre, or three different degrees of irritability of which it is ſuſceptible.

1. The *ſtate of health*, which is peculiar to each individual, and which I ſhall call

call the *tone* of the fibre, after a term of Stahl.

2. The *ſtate of accumulation*, produced by the abſence of habitual ſtimuli.

3. The *ſtate of exhauſtion* produced by the too powerful operation of ſtimuli.

The *ſtate of health*, or the *tone* of the fibre, conſiſts in a certain quantity of the irritable principle neceſſary to its preſervation. To maintain this ſtate, the action of the ſtimulus muſt be ſtrong enough to carry off from the fibre the ſurplus of this irritable principle, which the lungs and the circulation of the fluids are continually ſupplying. For this a certain equilibrium is neceſſary between the ſtimuli applied and the irritability of the fibre; in fine, that the ſum of all the ſtimuli acting upon it may be always nearly equal, powerful enough to carry off from the fibre the exceſs of its irritability, and not ſo ſtrong as to carry off more than this exceſs. It is in this equilibrium

librium between the acting ſtimuli and the irritability furniſhed by the lungs and the circulation, that the *health* or *the tone* of the fibre conſiſts. When the ſum of the ſtimuli acting upon the fibre is not great enough to carry off all its exceſs of irritability, the irritable principle accumulates in the fibre, and then it is found in that ſtate which I call the *ſtate of accumulation;* the irritable principle accumulates in the fibre, its irritability is augmented, and the ſtimuli produce much ſtronger contractions than when the fibre only retains its tone. Hence it is that in oppoſing an obſtacle to the movements of the hedyſarum girans for any length of time, the movement becomes conſiderably ſtronger after the obſtacle is removed.

When the ſum of the ſtimuli acting upon the fibre is too great, the fibre is deprived not only of the exceſs of its irritability but alſo of ſome portion of the irritable principle neceſſary for the tone of the fibre, or, more properly ſpeaking, the fibre loſes more irritability

irritability than it receives, and of courſe in a ſhort time finds itſelf in a *ſtate of exhauſtion*, and this exhauſtion will be either *temporary* or *irreparable*.

In the ſtate of *temporary exhauſtion* the fibre loſes its tone, and fails for want of irritability. The application of a ſtimulus, while it is in this ſtate, will not make it contract. Provided the ſtimulus be not very ſtrong, it will produce no effect at all, but in a ſhort time the irritable principle will accumulate afreſh in the fibre, and then it will again contract. It is only by little and little that the fibre recovers its irritability. This truth, I dare venture to ſay, is as new as it is ſtriking. It unfolds a vaſt number of phænomena hitherto inexplicable. Let us obſerve, for example, the motion of the heart; the heart contracts from the ſtimulus of the blood, and impels the blood through the arteries; it then again dilates, and the blood enters. But the heart does not contract itſelf immediately upon the firſt impreſſion of the blood. Its irritability

lity having been leſſened by the preceding contraction, it requires half or three quarters of a ſecond before the irritability of the heart ſhall have accumulated to ſuch a degree that the new ſtimulus can act upon it. It is impoſſible to explain the motion of the heart upon any other principle. Haller has indeed very well explained the motion, on the principle of the irritability of the heart; but he was never able to anſwer the famous objection of his opponents, who ſaid, If the blood acts upon the heart as a ſtimulus, and its contraction is the conſequence of ſuch action, how comes it that the heart does not contract as ſoon as the blood enters it, but that it flows in ſome time before the contraction is renewed? Why does not the effect immediately follow the cauſe? Haller could never anſwer this objection, nor ſeveral others of the like nature, inaſmuch as he was a ſtranger to the laws of irritability. The menſtrual diſcharge in women is explained on the ſame principle. The ſtimulus of the ovaries acting continually in women after the age of puberty (as I ſhall prove elſewhere) nevertheleſs does not produce its effects till the end of eight and twenty days; becauſe this period of time is neceſſary for the uterus, in its ſtate of health, to accumulate its irritability in ſufficient quantity for the ſtimulus to act; the diſcharge ceaſes after the irritability of this organ has been diminiſhed and returns with the returning irritability. All the periodical motions in animals and plants, as well as their periodical diſeaſes, may be explained upon the ſame principle; that is to ſay, any ſtimulus which is always preſent, and continually acting upon the fibre, produces no ſenſible effect, till the exhauſted irritability of the fibre ſhall have been accumulated afreſh. The periodical motions in organized bodies depend on the alternate exhauſtion and accumulation of the irritability of the fibre. A temporary exhauſtion of the irritability of the hedyſarum girans is produced by the heat of the ſun and by electricity, according to the obſervations of M. Brouſſonet. The electrical fluid exhauſts alſo the

the irritability of the mimoſa pudica for ſome time, as the abbè Bertholon has obſerved.

The total or irreparable exhauſtion of the fibre conſiſts in a total loſs of its irritability, as in the caſe of gangrene. The fibre changes its colour, becomes dark or black, and ſubject to the laws of inorganized matter, and begins to decompoſe and putrefy.

A very powerful ſtimulus will in a very ſhort time reduce the fibre to this ſtate. Such, for inſtance, is the ſtate of the fibre in animals killed by very ſtrong poiſons, by the bite of a rattleſnake ; in animals deſtroyed by a knife dipped in the juice of the aconite, or by poiſoned arrows. The irritability of many inſects, and of the greateſt part of plants, is irreparably exhauſted by the ſtimulus of the propagation of the ſpecies, ſo that they die the moment the work of generation is completed. Dr. Prieſtley has obſerved, that in expoſing plants to the ſtimulus of air in which animal ſubſtances

have

have putrefied, it always happened that, either the plants being vigorous enough to bear the action of the ſtimulus, their growth was very rapid, or the ſtimulus proved too powerful, and the plants died; their irritability was exhauſted in an inſtant, and their leaves became black and gangrenous.

The irritable fibre, from the firſt moment of its exiſtence to that of its diſſolution, being conſtantly ſurrounded by the body which acts upon it, and ſtimulates it, and upon which it re-acts by its contraction, it follows, that during the period of its exiſtence the irritable fibre is in continual action; and its exiſtence conſiſts in action; and that it is not a paſſive ſtate, as ſome authors have aſſerted. Hence, external objects having no immediate action upon the nerves, and only acting upon them, and producing their different ſenſations, through the medium of the irritable fibre, it is plain that the ideas we have of external objects are not conformable to thoſe objects, but that they are varied and modified by the irritable fibre through which they are tranſmitted to us. Objects, therefore, appear different according to the different ſtates of the fibre. The irritable fibres, which are combined together in every individual, whether animal or vegetable, form a ſyſtem of fibres, in which the integral parts act continually upon the whole, while the whole re-acts upon the parts, ſo that every ſtimulus which acts upon any fibre in the ſyſtem will deprive that part of its irritability; but this loſs will ſoon be repaired by the ſyſtem, and every fibre will furniſh, in proportion, ſome ſhare of its own irritability to ſupply the loſs in any one fibre. Thus it is that a very weak ſtimulus, but one that is conſtantly acting upon one part of the ſyſtem, ſuch as ſlow poiſons, the abuſe of ſpirituous liquors, &c. exhauſts in the end the whole ſyſtem, and produces death. For the ſame reaſon, a very powerful ſtimulus applied to one part of the ſyſtem, ſuch as laurel water, opium, the poiſon of the rattleſnake, will in an inſtant exhauſt all the irritability of the ſyſtem,

deſtroy

deftroy the animal, and leave the fibres without any irritability. I am convinced, from repeated experiments, that opium, alcohol, ammoniac, a folution of fugar of lead, fulphuric æther, deftroy animals by exhaufting the irritability of the whole fyftem, and that the mufcles of the animals deftroyed have, by the application of thefe ftimuli, been wholly deprived of their irritability. The effect was the fame when thefe ftimuli were applied to the mufcles and ftomach, and when injected into the veins of animals. I have alfo made very curious experiments with the fame fubftances upon vegetables.

The irritable fibres in the fame fyftem have not all the fame degree of irritability. They have different degrees of *capacity* for the irritable principle. The capacity of the fibres is in the ratio of their diftance from the heart. Thofe equally diftant have the fame capacity. Every ftimulus which affects one of the fibres affects the others at the fame time and in like manner. Hence the ſympathy of different and ſeparate parts and thoſe ſurpriſing phænomena which hitherto have been explained by the harmony of the nerves, although we ſee the ſame phænomena in the vegetable kingdom, which is deprived of nerves. Theſe ſympathetic phænomena are obſervable throughout organized nature. Whatever part of the polypus be touched, the whole will contract, and its arms will contract themſelves by ſympathy. If a worm be touched with the point of a pin, without wounding it, the whole worm will be ſeen to contract itſelf; which is a certain proof that the different parts are affected by ſympathy. If the ſlighteſt impreſſion be made upon one of the leaves of the averrhoa carambola, not only that leaf but all the neighbouring ones, and frequently ſome of the diſtant ones, will contract themſelves by ſympathy.

When the irritable fibre has loſt its tone, and fails, either from an exceſs of the irritable principle, or from a deficiency of this principle, it is diſeaſed, and the ſyſtem

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of which it forms a part fuffers and becomes difeafed through fympathy. All the difeafes of animals and vegetables may be ranged under two heads; to wit: Firft, the *difeafes of accumulation* caufed by the accumulation of the irritable principle, and by the diminifhed action of the habitual ftimuli. Secondly, the *difeafes of exhauftion*, caufed by a defect of the irritable principle proceeding from the encreafed action of the habitual ftimuli, or from the addition of new ftimuli. Under thefe two claffes may be ranged all difeafes whatever. Paradoxical as this propofition muft neceffarily appear to thofe who have not reflected on the fubject, it is neverthelefs true, and I fhall give the moft convincing proofs of it in a work I am about to publifh.

Remedies remove the difeafe by their action upon the irritable fibre, and by exhaufting its irritability, when the difeafe is that of accumulation; or by diminifhing the action of the common ftimuli, and confequently by preventing a total exhauftion, where

where the diſeaſe is that of exhauſtion. The effects of poiſons are to be explained in this way.

Poiſons, remedies, and in general all ſurrounding bodies acting only on the irritable fibre, it follows that they act upon the ſyſtem in a ſimilar manner, and that every ſubſtance capable of producing the greateſt poſſible effect upon the fibre, that is to ſay, every ſubſtance capable of exhauſting all the irritability both of the fibre itſelf and of the ſyſtem in an inſtant, as, for inſtance, laurel water, or white arſenic, is alſo capable of producing all the inferior degrees of action, either by acting upon a fibre leſs irritable, or by acting upon the ſame fibre, but in a leſs quantity. Laurel water, opium, white arſenic, ammoniac, are of courſe both medicines and poiſons capable of healing as well as of producing all maladies whatſoever without exception. And this is confirmed by a number of experiments which I have made upon different animals. This truth ſeems to me of the utmoſt im-

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portance ; and the abbé Fontana, who made more than ſix hundred experiments to prove that ammoniac is no remedy againſt the bite of the viper, would have ſaved himſelf a great deal of time and trouble, had he been acquainted with it. If inſtead of applying the venom of the viper to ſo many animals, and afterwards applying ammoniac to the wound, he had made a ſingle comparative experiment, and had applied ammoniac to a wound made by a lancet that was not poiſoned, he would have found that ammoniac itſelf, applied in this manner, would have produced a diſeaſe exactly analogous to that cauſed by the venom of the viper, and, conſequently, ſo far from removing the malady, muſt neceſſarily increaſe it, by exhauſting the irritability of the fibre in a much leſs time than the venom of the viper by itſelf was capable of doing. Mr. Fontana has made more than ſix thouſand experiments upon the poiſon of the viper; he employed more than three thouſand vipers, and cauſed to be bit more than four thouſand animals, and the concluſion he drew after this truly enormous number of obſervations was, that the poiſon of the viper kills all animals, and produces the diſeaſe by its action on the blood. But why did Mr. Fontana neglect to make the deciſive experiment, the *experimentum crucis* of Bacon. It is well known that frogs, and many animals with cold blood, live a long time without the heart, and entirely deprived of blood. If therefore the poiſon of the viper kills animals by its action on the blood, it will not deſtroy frogs without blood. But experiment contradicts this reaſoning. The poiſon of the viper will kill frogs without blood in as ſhort a time as it kills thoſe animals who have not loſt their blood. It is not therefore by its action upon the blood that the venom of the viper deſtroys animals; and thus does it happen that a ſingle experiment frequently overturns all that ſix thouſand other experiments have apparently eſtabliſhed. According to my experiments, poiſons operate upon the blood juſt as they do upon the muſcular fibre, by depriving it of its prin-

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ciple or irritability, or of its oxygene. After having made this obſervation upon the experiments of Mr. Fontana, I muſt do him the juſtice to add, that I have found all his experiments very accurate, and that in all thoſe which I have repeated, the reſult has been exactly conformable to the account given by him; it is in his concluſions only that he appears to be deceived.

The effect produced upon the irritable fibre by any ſtimulus, is in a ratio compounded of the degree of irritability of the fibre, and of the force of the ſtimulus. The ſame ſtimulus will produce greater contractions upon a fibre more irritable than upon one leſs irritable; and the irritability of the fibre being the ſame, it will contract itſelf more upon the application of a ſtronger than of a weaker ſtimulus.

The effect produced upon an irritable fibre by any ſtimulus is in the inverſe ratio of the repetition of its application. Cæteris paribus, the effect of any ſtimulus diminifhes every time its application is repeated, till at laft the effect is nothing, or $= 0$. This explains the phænomena of habit, and many other phænomena hitherto inexplicable in the animal and vegetable œconomy. The mimofa pudica, for example, expofed to a ftrong wind, contracts itfelf; but it ceafes to contract itfelf in obedience to this ftimulus after it has been accuftomed to it.

The effect produced upon the irritable fibre by any ftimulus, is in a ratio compounded of the degree of irritability of the fibre and the degree of the force of the ftimulus directly, and the degree of the habit of the fibre inverfely. Let the force or intenfity of the ftimulus $= a$, the degree of irritability of the fibre $= b$, the degree of the habit of the fibre $= e$, then the effect produced upon the fibre or x will $= \frac{ab}{c}$. But all the ftimuli acting in the fame manner, that which diminifhes the irritability of the fibre for a certain ftimulus,

lus, will in the like manner diminiſh it for the ſtimulating force in general, wherefore the habit of the fibre is comprehended under its degree of irritability, or *c* is comprehended under *b*. Therefore *x* will $= ab$.

The effect produced upon the irritable fibre by any ſtimulus, or *x*, being always equal to *ab*, it follows that the value of *a* and *b* being known, the value of *x* is known. But admitting an unity fixed and conſtant, it will be eaſy in all caſes to expreſs by numbers the degree of irritability of the fibre and the degree of the force of the ſtimulus, or the value of *a* and *b*, conſequently it will be eaſy to find the value of *x*. All the art of medicine then conſiſts in the art of finding the value of *x*, that is to ſay, in finding a ſtimulus adequate to reſtore the tone of the fibre. Thus, if theſe principles be true, phyſic, which at preſent is an art of mere conjecture, will be reduced in time to the certainty of calculation, and after tables ſhall be formed to expreſs the values of *a* and *b*, and the ſigns by which they

they may be known, this calculation will be ſo ſimple and eaſy, that it will from a part of the education of every individual. But further, the irritable fibre being the ſame in all organized nature, diſeaſes and their remedies will of courſe be the ſame for all organized beings: there will then be no diſtinction between medicine, farriery, and agriculture, but all theſe ſciences will be confounded, and become one, under the general name of *univerſal phyſiology.* The art of pharmacy and the ſcience of preſcription-writing will become uſeleſs; a phial of alcohol or laudanum will ſupply the place of that enormous quantity of drugs which crowd the ſhops of apothecaries. The trade of the druggiſt—but hold, if I continue this prophetic language, I ſhall only expoſe myſelf to ridicule; for, as Helvetius obſerves, 'every idea very foreign to our habits of ſee-'ing and thinking appears ridiculous to us. 'We never value any ideas but what are analo-'gous to our own, becauſe we are under the 'neceſſity of eſteeming ourſelves only in 'others.'

Thoſe

Thoſe ſtimuli which I call common and habitual, becauſe they act continually more or leſs upon the irritable fibre, are, *heat*, *light*, *nouriſhment*, *air*, *the circulation of the blood*, *the ſtimulus of generation*, and *the nervous ſtimuli*. So long as the action of theſe ſtimuli is in proportion to the degree of irritability of the ſyſtem, and the ſum of their action is nearly equal to the ſum of the irritable principle abſorbed by the lungs, and diſtributed by the circulation, the whole ſyſtem will be in proper order, and the conſtituting fibres will have their tone. When one or more of theſe ſtimuli act more powerfully than ordinary, or the fibre becomes more irritable, while the degree of the action of the ſtimuli remains the ſame, the *exhauſtion* of the ſyſtem, and one of the diſeaſes in its train, will be the conſequence.

The abſence of one or more of theſe ſtimuli will produce an *accumulation* of irritability in the ſyſtem, and give birth to one of the diſeaſes of this claſs. I ſhall ſpeak

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of all thefe ftimuli feparately, in order that I may be better able to explain myfelf.

Of heat.—The heat of the atmofphere, and of all furrounding bodies, acts upon the fibre and ftimulates it. I am convinced of the ftimulating action of heat from direct experiments. I have expofed fmall animals, fuch as cats, dogs, rabbits, &c. in covered veffels, to the heat of boiling water, which furrounded the veffel in which the animal was placed, fo that the water could not touch it. Animals deftroyed by heat in thefe experiments, upon diffection have been found to have loft all their irritability. Their heart and mufcles contracted themfelves but feebly, even upon the application of the ftrongeft ftimuli, fuch as electricity. It is proved by fome beautiful experiments of Mr. Hope, that heat acts as a ftimulus upon plants; and it is obfervable that plants expofed to the fun are larger, and produce more flowers and fruit than thofe which are lefs expofed to heat. Trees in general are more luxuriant which grow in the ſouth than thoſe in the north. This is a proof that heat is a ſtimulus to the irritable fibre. The diſeaſes of hot climates are all the diſeaſes of exhauſtion, cauſed by the too powerful action of the ſtimulus of heat. Hence the cuſtom of taking ice in hot countries to reſtore the tone to the fibre, by abſorbing the heat and preventing its ſtimulating action. This irritability of the hedyſarum gyrans is exhauſted by the heat of the noonday ſun, according to the obſervations of M. Brouſſonnet; and by the experiments of M. Fontana and M. Medicus it is proved that the irritability of plants is great in the morning, diminiſhed during the heat of the day, and little or none in the evening.

Of cold.—Cold being a leſs degree of heat, its effects upon the irritable fibre are in proportion to the *habit*, or the quantity which is neceſſary to the fibre to preſerve its tone. The animals and plants of hot climates, that require the ſtimulus of a great heat to preſerve the tone of their leſs irritable fibres, are affected by the leaft abftraction of this habitual ftimulus; the irritability of their fibres accumulates in confequence of this abftraction, and the return of the heat again exhaufts the fibre. The more intenfe the cold is, the greater is the accumulation of irritability After the fibre has been expofed for fome time to a great degree of cold, its irritability is increafed to fuch a degree, that the moft trifling degree of heat produces the moft violent effects: hence the glow experienced in coming out of a cold bath; hence the difeafes which are caught in coming out of the cold air into a warm room, and which medical men attribute to checked perfpiration, a fuppofition entirely falfe.

The leaft movement is attended with fatigue upon the fummit of high mountains, as I have frequently experienced, but efpecially in 1785, upon the top of the Buet, and, as M. Sauffure has likewife obferved, upon the fummit of Mount Blanc. The reafon of it is this; the fibre is rendered fo irritable

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by the cold of thofe mountains, that the leaft motion of the mufcles, or what is the fame thing, the leaft action of the nervous ftimuli, exhaufts it. It is by the gradual application only of heat that frozen limbs can be recovered, and it is neceffary always to begin by rubbing them with fnow; without this the fibre will be exhaufted, and become gangrenous. During the winter, by the abfence of the ftimulus of heat, and in part of light, plants and many animals become torpid, the organs of circulation and of nutrition perform their functions but languidly, and life itfelf appears fufpended. In confequence of the diminifhed action of thefe ftimuli, the irritability accumulates, and manifefts itfelf at the return of fpring. The leaft degree of heat then produces the moft violent effects upon the fibres thus delicately irritable. Animals, which had concealed themfelves under ground, venture forth from their fubterraneous retreats, plants put forth their leaves and flowers, and man himfelf is fenfible of the ftimulus of heat in the gales of fpring,

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his fibre being rendered more irritable by the winter's cold. Vegetation is much more vigorous in ſpring time than during the reſt of the year. It diminiſhes during ſummer in proportion as the irritability accumulated during winter is diminiſhed by the action of heat and light, and, laſtly, is exhauſted in the autumn. Dr. Hales obſerved, that the rapidity with which the ſap circulates in the vine during ſpring is five times greater that the rapidity with which the blood flows in the arteries of a horſe. This motion is much ſlower in ſummer, and almoſt ceaſes in autumn. It is not the effect of the heat alone, for if that were the caſe it would increaſe as the heat increaſed, and the effect would be proportionate to the cauſe; it is the effect of the irritability accumulated in conſequence of the abſence of heat during the winter. The effects of winter are very great in cold climates, becauſe the accumulation of the irritability is in proportion to the abſtraction of the ſtimulus of heat. In Lapland corn ripens in ſixty days, whereas in France it requires an hundred and twenty, or an hundred and thirty days. The truth of what is here advanced may be proved by expofing vegetables alternately to heat and cold: it is furprizing how much their growth and the power of vegetation is increafed. But in thefe experiments care muft be taken to vary the temperature by degrees; becaufe the irritability accumulating in the fibre by the abftraction of the heat, a very fmall quantity of this ftimulus then applied is fufficient to exhauft it entirely, or to deftroy it. Hence it is that the return of cold and froft in the beginning of fpring is fo noxious to vegetables, and that the year is in general more abundant after a very cold winter. Mr. Fontana obferved, that during winter the vipers which he kept for his experiments were in a torpid ftate, though the thermometer was at 59°. He endeavoured to render them vigorous by warmth, and expofed them to a heat of 67° only. In two minutes they died, though during fummer they bear a much greater degree of heat; but then they are lefs irritable. Spallanzani obferved

that

that newts bury themſelves in the earth, and become torpid, in the month of October, before the thermometer in the ſhade falls to 54½, and that they re-appear in the month of February, though at that time it freezes every night, and frequently during the day the thermometer is many degrees below 54°. What is the reaſon, enquires this excellent obſerver, that theſe animals revive in ſpring, when the cold is more intenſe, and ſink into torpidity at a much leſs degree of cold in autumn? I will ſolve this problem, by obſerving that in autumn a very great ſtimulus is required to act upon the fibre of theſe animals, exhauſted as it has been by the heat of the ſummer; but in ſpring, the leaſt ſtimulus, the leaſt increaſe of heat, is ſufficient to put the fibre in action, its irritability having accumulated during winter in conſequence of the abſence of the common ſtimuli.

Light is another common ſtimulus. To convince myſelf of the ſtimulating quality of light upon plants by direct experiments, I enveloped

enveloped the leaves of ſome plants in an opake body, ſo that the air might have free acceſs, while the light could not penetrate. I found that theſe leaves became more irritable than the others, the irritability having accumulated. By the abſtraction of the ſtimulus of light, the irritability of organized bodies accumulates, and a diſeaſe enſues, which is called *étiolement.* Animals deprived of light, and living in dark places, loſe their colour and become white, as is obſervable in arctic animals during the long nights in the countries near the pole: I have obſerved it alſo in the animals that inhabit the Alps, and which conceal themſelves for the greateſt part of the year in ſubterraneous dwellings. *Blanched* plants loſe their green colour, and become whitiſh and ſickly. Some poiſonous plants loſe their noxious qualities, and become agreeable to the taſte, merely by the abſtraction of the ſtimulus of light. White animals and plants are very irritable; and it is obſerved that theſe animals and plants are not capable of ſupporting a great quantity of light.

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The action of the light upon plants has been very well obſerved by Dr. Ingenhouſz and Mr. Senebier, and the manner in which colours are produced has been explained by M. de la Metherie. It is well known that animals that have been tamed, and eſpecially domeſtic animals, change their colour by education; but an obſervation that has perhaps eſcaped naturaliſts is, that this change is conſtantly from dull colours to thoſe that are brighter or leſs dull. I have often obſerved, that the change takes place more frequently in dark than in light places. Mice kept in a cage in a dark room have produced white mice.

The third common ſtimulus *is that of nutriment.* It requires a very ſmall quantity to ſupply the daily loſſes; the greateſt portion is employed in depriving the ſtomach, and of courſe the whole ſyſtem, of its ſuperfluous irritability. This is proved by what is obſerved in organized bodies. All animals are more irritable before than after food. Hunger, of which appetite is the leaſt degree, is cauſed by the accumulated irritability of the ſyſtem. The gaſtric juice acts upon the fibres of the ſtomach now become more irritable, and produces the ſenſation of hunger. Spallanzani has obſerved that birds of prey do not void indigeſtible bodies, ſuch as pieces of glaſs or metal, which they have taken in with their food, before their ſtomach is empty. Theſe indigeſtible bodies cannot be voided while the ſtimulus of the nutriment acts upon the ſtomach; but as the abſtraction of this ſtimulus gives the irritability of the ſtomach an opportunity of accumulating, the indigeſtible bodies very ſtrongly ſtimulate the fibres of the ſtomach, make them contract, and by this contraction they are voided. It is poſſible to do almoſt wholly without nutriment, by applying from time to time ſome other ſtimulus to the ſtomach, ſuch as tea, coffee, alcohol, opium, and by exhauſting by theſe means the accumulated irritability of that organ. By the entire abſtraction of the ſtimulus of nutriment, the irritability of the ſyſtem is prodigiouſly increaſed. There are many inſtances of perſons who, not having eaten any thing for many days, have been intoxicated, and killed, in conſequence of ſwallowing, with great greedineſs, two or three cups of broth. Plants ſuddenly tranſplanted from a meagre, into a very rich ſoil, produce no fruits or ſeeds, and die in a ſhort time of a particular diſeaſe, cauſed by exceſs of nutriment.

The *circulation of the fluids* is the moſt powerful of the common ſtimuli. The blood, which oxygenates itſelf during its paſſage through the lungs, parts with its oxygene in the circulation, the oxygene having a ſtronger attraction for the irritable fibre than for the carbon which is contained in the blood. In this operation the heat combined with the oxygene is ſet free. Hence animal and vegetable heat. The blood acts continually upon the irritable fibre, and the fibre re-acts upon the blood, and this action and re-action are ſtronger in proportion

proportion as the circulation is more rapid, and as the air which comes in contact with the blood in the lungs contains more oxygene air.

When any local ſtimulus continues to act upon any part of the ſyſtem, the circulation becomes more rapid, and a fever is the conſequence. Is the ſtimulus weak, a ſlow fever enſues, which will by little and little exhauſt the irritability of the ſyſtem, and the patient will die of a conſumption. Is the ſtimulus ſtronger, or the fibre upon which it acts more irritable, we ſhall have an ardent fever, which will exhauſt the irritability in a leſs time. In fine, is the ſtimulus very violent, or the fibre diſeaſed by an exceſs of irritability, we ſhall have a putrid fever, which will deſtroy the patient, whether animal or vegetable, and will exhauſt the irritability in a very ſhort time. But whatever be the nature of the fever, the fibre irritated by the ſtimulus will act upon the blood more than ordinarily,

rily, the re-action of the blood will be increafed in proportion, the circulation will be more rapid, the blood will abforb more oxygene, and the whole fyftem will be furcharged. By this means the irritability will be increafed, the animal heat augmented, and the effect of the action of the ftimulus becoming greater in proportion to the accumulation of irritability, a total exhauftion of the irritability, or the death of the patient, will enfue. There are two methods of preventing the fatal effects of a local ftimulus, whofe operation upon one part of the fyftem is conftant. The firft confifts in preventing the furcharge of oxygene in the blood, which is accomplifhed by diminifhing the proportion of oxygene gas in the air breathed by the patient, or by diminifhing the quantity of blood by phlebotomy. The fecond method confifts in applying ftimuli capable of exhaufting the irritability in proportion as it accumulates; fuch as wine, opium, bark, heat, &c. Phlebotomy acts by diminifhing the

the quantity of blood, and consequently its operation is attended with this effect, viz. of diminishing the re-action, and restoring to the fibre its tone. I shall here observe by the way, that the advice which many physicians have given, to make the patient breathe oxygene gas, is the most pernicious they can give; for the patient always finds himself worse after having breathed this salutary gas, as I have frequently had occasion to observe.

The *nervous stimulus* is the only one which is peculiar to animals. It is this stimulus which is the cause of the voluntary motions, of convulsions, and passions. The passions differ from one another only in stimulating the irritable fibre more or less. Angér and joy are very powerful degrees of the nervous stimulus; content and hope are weak degrees; fear, sorrow, fright, despair, are not absolute degrees of this stimulus, they are only the abstraction of the stimuli of hope, content, and happiness. Anger and

joy

joy act as very powerful ſtimuli, and exhauſt the irritability of the fibre in the ſame manner as any other ſtimulus whatever. Content and hope are degrees of the nervous ſtimulus, neceſſary to preſerve the tone of the fibre. Sorrow and fright are degrees too weak. If they continue to act, the irritability of the fibre accumulates. It is well known that fearful and melancholy perſons are oftener affected by the ſtimulus of contagious diſeaſes than they who are free from fear, and who take the precaution of applying a greater quantity of ſtimulus than ordinary to their fibres, by taking wine, vinegar, opium, and bark. According to the obſervations of Mr. Fontana, timid and fearful animals die much ſooner of the bite of the viper than courageous or irritated animals. Joy excited by the annunciation of good news to a ſorrowful perſon, and one of courſe very irritable, has often cauſed death. The ſtory of the Roman mother is well known, who was bewailing the death of her ſon, and who dropt

dropt down dead for joy the moment ſhe ſaw him enter her room alive.

By the abſtraction of many of the common ſtimuli for any length of time, the irritability of the fibre accumulates ſo much, that the moſt trifling ſtimulus produces the moſt violent effects, and frequently even inſtantaneous death. This diſeaſe is called the ſcurvy, concerning the nature of which medical men have formed ſo many falſe and ridiculous theories. It is of the utmoſt importance to mankind to know the true nature of this diſeaſe; ſince, in conſequence of our ignorance in this particular, we have been unable to find a ſure remedy for it, and ſo many thouſands of lives have fallen a ſacrifice to its ravages, in armies, fleets, and beſieged towns. In the laſt war the Engliſh fleet ſuffered dreadfully from the ſcurvy; and laſt year a great number of ſoldiers died of this diſeaſe in the imperial army in Walachia, in conſequence of the abſtraction of the ſtimulus of nutriment (the emperor having

having ordered that a kind of paſte made of bread and water ſhould be given to the ſoldiers inſtead of meat), of the ſtimulus of oxygene, in the corrupted atmoſphere of the fens of Walachia, and laſtly, of the nervous ſtimulus, the moſt powerful of all; for the greateſt part of the army were engaged by force, and againſt their wills.

The abſtraction of all theſe ſtimuli accumulated the irritability of the fibre, and cauſed the ſcurvy, and that dreadful mortality that took place in the army. The ſame cauſes produce the ſame effects upon animals. We ſee domeſtic animals affected with the ſcurvy in conſequence of cold and hunger, that is to ſay, in conſequence of the abſtraction of the ſtimuli of heat and nutriment.

The ſheep which captain Cook had on board his ſhip, on his voyage round the world in the years 1772, 3, and 4, died of the ſcurvy, their teeth fell out, their gums rotted; in a word, they had all the ſymptoms of an inveterate ſcurvy. The abſtraction of the common ſtimuli in plants produces ſimilar ſymptoms and a ſimilar diſeaſe. The diſeaſe of rye called *ergot* is exactly analogous to the ſcurvy in animals; the *ergot* is the ſcurvy of plants; it is the effect of accumulated irritability in the fibres of plants. The cauſes which produce the ergot of rye are the ſame as thoſe which produce the ſcurvy in animals. According to the obſervations of Saillant and Teſſier, theſe cauſes are, a wet and barren ſoil, and a cold ſummer; that is to ſay, the cauſes of the ergot are, the abſtraction of the ſtimuli of nutriment and heat. I could enlarge upon this intereſting ſubject, if I were not afraid of making this eſſay too long. I wiſhed to give only the outlines, or a general view of my theory, without entering into the detail. In the ſubſequent eſſays I ſhall treat of oxygene conſidered as the principle of irritability, of the compoſition and decompoſition of water in animals and plants, of the different kinds of air contained in the interior cavities of organized bodies, and of the circulation of this air, the exiſtence of which has not hitherto been even ſuppoſed, although, as I ſhall prove hereafter, the lymphatics in animals, and the fibres in plants, are almoſt ſolely deſtined for the circulation of theſe elaſtic fluids.

MEMOIR

MEMOIR II.

HAVING given this general ſketch of a new ſyſtem of phyſiology, founded upon a number of experiments ſhewing that irritability is the principle of life, I proceed to prove that oxygene is the principle of irritability; that it unites with the blood in the lungs during reſpiration; that it is diſtributed to every part of the ſyſtem by the circulation, and that it combines with ſtimulating ſubſtances, with which the different parts of the ſyſtem come in contact.

I think that the oxygene is abſorbed by the blood, and that the venous blood is oxygenated in the lungs during reſpiration. The moſt celebrated naturaliſts and chymiſts are of a different opinion; they think that the oxygene does not combine with the venous blood. According to them this laſt loſes carbone and hydrogene, and recovers the bright colour natural to it, without abſorbing any thing from the atmoſphere.

Here are ſome experiments and reaſons, upon which their theory of reſpiration is founded.

1. Arterial blood expoſed to the contact of hydrogene air loſes its vermilion colour, and aſſumes the black and deep appearance of venous blood. The hydrogene air is abſorbed in part in this experiment.

2. Mr. Hamilton made three ligatures in the jugular vein of a cat. Having expelled the blood from between two of the ligatures, he then introduced the hydrogene air, and kept it there, cloſing the aperture through which he had introduced it. He then looſened the middle ligature, and the blood contained between that and the third ligature came into contact with the hydrogene

gene air. In about an hour, having taken the blood from the vein, he found it liquid, and of a colour almoſt as black as ink.

3. Venous blood expoſed to vital air acquires the vermilion colour of arterial blood, and the air is rendered impure.

Mr. Lavoiſier and Dr. Crawford (Annales de Chimie, tom. V. p. 267) have drawn the following concluſions from theſe experiments:

1. That the change of colour which the blood undergoes during circulation proceeds from its combination with hydrogene air.

2. That in paſſing through the lungs, the blood parts with a portion of the hydrogene it contains, and then re-aſſumes its vermilion colour.

Mr. Lavoiſier and Dr. Crawford think, that during reſpiration the vital air which is is received into the lungs combines with the carbone and hydrogene that is disengaged from the blood; that it forms carbonic air with carbon, and water with hydrogene; and that the blood recovers its vermilion colour after it has loſt the carbon and hydrogene with which it had been charged during the circulation.

Without preſuming to contradict philoſophers of ſuch diſtinguiſhed merit, I cannot help obſerving, that it appears to me that theſe concluſions do not neceſſarily follow from the experiments, and that they are to be explained in a manner more conformable to the laws eſtabliſhed by modern chemiſtry. I know no experiment which authorizes us to ſuppoſe that carbone can unite with oxygene in a temperature of 97°—99°, or that hydrogene and oxygene air combine and form water in ſo low a temperature. M. Seguin has attempted to anſwer this objection, by ſuppoſing that the carbon is in a very attenuated ſtate in the blood, and by citing the experiments of M. Ber-

M. Berthollet upon hydrogene air. But this explanation appears to me hypothetical, and no way convincing.

After having a long time attended to the phænomena of reſpiration, and made many experiments upon this ſubject, I think it may be concluded, that during reſpiration one part of the oxygene of the vital air combines with the venous blood, of which it changes the black colour and makes it vermilion*; the ſecond part of the oxygene unites with the carbon contained in the carbonic-hydrogene gas, which exhales from the venous blood, and forms carbonic acid air; a third part of the oxygene unites with the carbon of the mucus, contained in great quantities in the lungs, and which is continually decompoſing; this part alſo forms carbonic acid air; a fourth part of the oxygene combines with the hydrogene of the blood to form water, which is exhaled during reſpiration. The

* Dr. Goodwyn had proved this before. Could Dr. Girtanner be unacquainted with his experiments?

heat

heat contained in the vital air being decompofed, remains united in part with the oxygene and the blood. Hence the quantity of heat peculiar to the arterial blood, which is much greater than that of the venous blood. Another part of the heat enters into combination with the carbonic acid air. Laftly, a third part produces a temperature neceffary for the formation of water, by the combination of the hydrogene and oxygene airs.

The effects of refpiration will confequently be thefe:

1. The venous blood lofes the carbonic-hydrogene air which it contains, and abforbs the oxygene air, which gives it its vermilion colour, fuch as it gives to metallic oxids, nitrous acids, and many other fubftances with which it enters into combination.

2. The capacity of the blood will be increafed, becaufe oxygene increafes the

 capacity

capacity of all ſubſtances to which it unites.

3. The oxygene air of the atmoſphere is partly abſorbed by the venous blood, changed in part into carbonic acid air by the carbone of the blood and that of the mucus of the lungs, while the reſt forms water with the hydrogene air of the blood, and a quantity of heat is ſet free.

The products of reſpiration will be—

1. Animal oxid, fluid (i. e. arterial blood.)

2. Carbonic acid air.

3. Water.

4. A ſmall quantity of liberated heat.

Nothing is more eaſy than to explain by this theory the experiments above related.

If we expoſe, under a veſſel filled with hydrogene air, arterial blood to the contact of this air, the quantity of air will be diminiſhed, loſe its vermilion colour, and become livid. In this experiment exactly the contrary takes place of that which is obſervable in reſpiration. The hydrogene air unites with the oxygene of the arterial blood to form water, and the arterial blood, being deprived of its oxygene, becomes black, and is changed into venous blood; the deep colour which it aſſumes proceeds from the loſs of its oxygene alone. The experiment of Mr. Hamilton proves this. He adds, that he found the blood liquid and very little coagulable. This is another proof in my favour. I have ſaid, in my former eſſay, that the coagulability of fluids obeys the ſame laws, and depends upon the ſame principle as the irritability of the ſolids; conſequently, the blood deprived of the irritable principle, or of oxygene, ought to be liquid, that is, to poſſeſs little or no coagulability.

The third experiment is a direct proof that

that the vermilion colour of the blood is owing to the abſorption of oxygene.

Having ſhewn that the arguments upon which the generally received theory of reſpiration is founded are not concluſive, I proceed to ſtate ſome direct experiments in favour of the new theory which I wiſh to eſtabliſh.

A. Experiments upon venous blood.

1. Six ounces of black venous blood, taken from the jugular vein of a ſheep, were introduced into a veſſel filled with oxygene air, and in an inſtant the blood aſſumed a vermilion colour; the thermometer within the veſſel roſe ſeveral degrees, but ſunk again immediately. The mercury in which the veſſel was placed roſe from ſix to eight lines. When the experiment was finiſhed, the blood was increaſed a little in weight; but though I am certain of this increaſe of weight from repeated experiments, I cannot

 exactly

exactly aſcertain how much it was, becauſe the inſtruments I made uſe of for this purpoſe were not ſufficiently exact for ſo delicate an experiment. The oxygene air which the veſſel contained was mixed with carbonic acid air, which lime water abſorbed. Some drops of water were formed at the bottom of the veſſel.

This experiment proves, that during reſpiration the blood abſorbs the oxygene; and I make no doubt but it is poſſible to determine the weight of the oxygene abſorbed, by repeating this experiment with inſtruments as exact as thoſe of M. Lavoiſier.

This experiment alſo proves, that during reſpiration there is formed carbonic acid air and water, that is to ſay, that there is an exhalation of the hydrogene air from the blood *.

2. The jugular vein of a ſheep was opened, and the blood which flowed from

* Rather of the baſe of hydrogene air.

it

it was received in a glafs bottle filled with oxygene air. The bottle, when half full, was clofed. The blood which it contained immediately affumed a vermilion colour, became very fluid, and coagulated but flowly into a reddifh and thick mafs, without any feparation of ferum. On the morrow the bottle was opened in order to examine the air which it contained, and the oxygene air was mixed with carbonic acid air. Some drops of water were formed near the mouth of the bottle.

This experiment confirms the firft.

3. A confiderable quantity of very pure oxygene air was injected into the jugular vein of a dog. The animal raifed moft terrible outcries, breathed very quickly, and with the utmoft difficulty; by little and little his limbs became hard and ftiff, he fell afleep, and died in lefs than three minutes. Upon opening the thorax and the pericardium, the heart was found more irritable than ordinary, and its alternate contractions and di-

latations

latations continued upwards of an hour. The right auricle of the heart was vermilion, and it contained, as well as the right ventricle, a great quantity of blood of a bright vermilion colour, frothy and not coagulated. The blood contained in the left ventricle, in the aorta, and the arteries, was of a rofe colour, and was mixed with bubbles of air. All the mufcles were more irritable than ordinary. After the blood contained in the heart and veins was difcharged, the irritability of the heart and the mufcles fenfibly diminifhed.

This experiment appears to me to prove moft decifively, that the vermilion colour which the blood affumes in paffing through the lungs is not owing to the lofs of the carbonic-hydrogene air, but that it proceeds from the combination of the blood with the oxygene air. In the experiment I have defcribed, the livid colour of the venous blood in the right auricle and right ventricle of the heart was changed to vermilion. Neverthelefs it could not have loft

any

any carbonic-hydrogene air; it only acquired oxygene. Befides, this experiment is a direct proof that oxygene is the principle of irritability; for by furcharging the blood with oxygene, by hyper-oxygenating it, if I may ufe the expreffion, the irritability of the blood was, as we have feen, confiderably increafed.

4. A fmall quantity of azotic air, which had been expofed for fome time to the contact of lime water, in order to feparate any carbonic acid air it might contain, was injected into the jugular vein of a dog. The animal died in twenty feconds. Upon opening the thorax, the pericardium, and the heart, the right auricle and ventricle were filled with black thick and coagulated blood. The left ventricle was of its ordinary colour. The heart, and almoft all the mufcles, loft their irritability almoft intirely; they contracted but weakly upon the application of the ftrongeft ftimuli, fuch as fulphuric æther and the electric fpark.

5. The

5. The venous blood of a ſheep was received in a bottle filled with azotic air. The blood coagulated in an inſtant, and aſſumed a colour black as ink. There was a ſeparation of a great quantity of ſerum. The next day, on opening the bottle, a faint ſmell of ammoniac was perceivable. The air was azotic air, which extinguiſhed a light.

In this experiment, the azotic air in contact with the venous blood rendered its colour deeper, and even quite black. The ammoniac produced is owing to the hydrogene air which eſcaped from the venous blood, and united itſelf to the azote.

The colour of the blood becoming deeper after it had loſt part of the hydrogene united with it, ſeems to prove that this deep colour is owing to the carbon of the blood, and not to the combination of hydrogene air, as has been ſuppoſed.

6. A bottle full of carbonic acid air was

half filled with the venous blood of a ſheep. It coagulated in an inſtant, aſſumed a very deep colour, and there was the ſeparation of a great quantity of reddiſh ſerum.

7. A ſmall quantity of carbonic acid gas was injected into the jugular vein of a dog. The animal became ſleepy, and died in about a quarter of an hour. The right auricle and ventricle of the heart were filled with thick blood, and in part coagulated. The blood contained in the left ventricle and auricle was of a deeper colour than ordinary. The heart and muſcles had loſt all their irritability.

This experiment proves, moreover, that the deep colour of the venous blood is not owing to the combination of hydrogene air. In this experiment, part of the oxygene of the carbonic acid air probably unites itſelf with the hydrogene of the blood, and forms water, and the carbon, which before was combined with this oxygene, unites with the blood, and gives it its deep colour.

8. An incifion was made in the jugular vein of a fheep, and the blood which came from it was received in a bottle full of nitrous air. When the bottle was half filled, it was clofed. The blood coagulated immediately, and a feparation of a great quantity of blackifh ferum took place. The day after, on opening the bottle, a very ftrong fmell of nitrous æther (dulcified fpirit of nitre) was perceived; the nitrous air having been changed in part to nitrous æther by the carbonic-hydrogene air of venous blood.

This experiment proves beyond a doubt that the venous blood contains carbonic-hydrogene air, and that this air is not very intimately united with it, but that it feparates with the greateft eafe. The nitrous æther produced in this experiment is owing to the union of the carbonic-hydrogene air, which exhales from the blood, with the nitrous air. The blood, after it has loft this air, does not affume a vermilion colour; but, on the contrary, it takes a very deep colour; it is not, therefore, to the union of the

the blood with carbonic-hydrogene, that the deep colour of the venous blood is owing, ſince this colour becomes ſtill deeper when the hydrogene is ſeparated from the blood.

9. A ſmall quantity of nitrous air was injected into the jugular vein of a dog. The animal died in leſs than ſix minutes. The right auricle and ventricle of the heart were filled with blood, thick, black, and partly coagulated. The blood contained in the left ventricle of the heart, was of a much deeper colour than ordinary; the heart had loſt its irritability. The lungs were of a greeniſh caſt, and partly putrefied *. All the canal of the wind pipe was filled with a green foam, that came in great quantities out of the mouth of the animal during the convulſions that preceded its death.

Experiments upon arterial blood.

10. An inciſion was made in the carotid

* The green colour is a ſign of nitrous acid, not of putrefaction. B.

artery of a ſheep, and the blood that iſſued thence was received into a bottle full of oxygene air. The bottle, when half filled, was cloſed. The colour of the blood became in an inſtant of a bright vermilion. The next day the bottle was opened, and the oxygene gas which it contained was found mixed with a very ſmall quantity of carbonic acid gas.

11. The arterial blood of the carotid artery of a ſheep was received into a bottle full of azotic air. The bottle, being half filled, was cloſed; the blood coagulated at the ſame moment, and aſſumed a very deep colour. On opening the bottle the next day, the azotic air which it contained was found mixed with a ſmall quantity of oxygene air, ſo that a candle burnt in it for near two minutes.

This experiment proves deciſively,

1. That arterial blood contains oxygene air.

2. The

2. That it is to its combination with this air that its vermilion colour is owing; and that it affumes its deep colour as foon as it is deprived of its oxygene air.

12. Three ounces of vermilion blood, from the carotid artery of a fheep, were received upon a plate, which was immediately placed under a veffel filled with carbonic acid air. The blood did not change its colour, but continued the fame for fome hours.

13. Arterial blood, from the carotid artery of a fheep, was received into a bottle filled with carbonic acid air. No change in the vermilion colour.

Thefe two experiments prove that carbonic acid air has no action upon arterial blood, although it has a very great one upon venous blood.

14. The arterial blood of the carotid artery of a fheep was received into a bottle

full

full of nitrous air. The bottle, when half filled, was clofed. The blood contained in it coagulated immediately, and affumed a green colour upon the furface. A fmall quantity of greenifh ferum was feparated. The day after, on opening the bottle, the vapours of nitrous acid were obferved by all who were prefent.

Here then is an experiment which proves, in a moft decifive manner, the prefence of oxygene in the arterial blood; fince it is from this circumftance alone that it is capable of changing nitrous air into nitrous acid. The green colour, obferved in this and the 9th experiment, arifes from part of the azote feparating itfelf from the nitrous air.

15. Arterial blood, from the carotid artery of a fheep, was received into a bottle full of hydrogene air, which was clofed when half filled. The blood became of a brighter vermilion, and remained fluid for fome time. It coagulated at laft, and a fmall quantity of ferum

ſerum was ſeparated. The day after, the hydrogene air contained in the bottle was found mixed with a ſmall quantity of oxygene air, which the nitrous air abſorbed. This experiment proves the preſence of oxygene air in arterial blood.

16. The arterial blood, as before, was received in a bottle containing equal portions of oxygene and hydrogene air. The bottle, when half filled, was cloſed. The blood in the bottle became moderately hot, remained fluid, and was of a more vermilion colour. It coagulated at laſt, and a ſmall quantity of ſerum was ſeparated. The day after, the air in the bottle was mixed with a ſmall quantity of carbonic acid air, of which the preſence was aſcertained by lime water.

17. A ſmall glaſs tube was filled with arterial blood of a bright vermilion; it was ſealed hermetically, and expoſed to the light. The blood changed its colour by

by degrees, and in ſix days became as black as venous blood.

18. The ſame experiment was repeated, with this difference only, that the tube was expoſed to heat, and not to the light. The blood became black in a ſhorter time.

The 17th and 18th experiments made by Dr. Prieſtley, and repeated afterwards, appear to me to demonſtrate, that it is not to the contact with hydrogene air that the venous blood is of a black colour.

I conclude from theſe experiments,

1. That the change of colour the blood undergoes during circulation is not owing to its combination with hydrogene air.

2. That the deep colour of the venous blood, is owing to the carbon it contains.

3. That the vermilion colour of the ar-

terial blood proceeds from the oxygene with which the blood is combined, during its paſſage through the lungs.

4. That reſpiration is a proceſs exactly analogous to the combuſtion and oxidation of metals; that theſe phænomena are the ſame, and to be explained in the ſame manner.

5. That during circulation, the blood loſes its oxygene, and charges itſelf with carbonic-hydrogene air, by means of a double affinity.

6. That during the diſtribution of the oxygene through the ſyſtem, the heat which was united with this oxygene eſcapes; hence the animal heat.

7. That the great capacity of the arterial blood for heat is owing to the oxygene with which it is united in the lungs.

Having ſhewn that the blood is oxygenated in its paſſage through the lungs; that in the circulation it loſes the oxygene it had abſorbed ; and that it returns to the lungs ſurcharged with carbonic-hydrogene air, it remains to prove, that to this oxygene, diſtributed through every part of the ſyſtem, is owing the irritability and the life of organized bodies. Here follow the proofs upon which this theory is founded.

The irritability of organized bodies is always in a direct ratio of the quantity of oxygene they contain.

1. Every thing that increaſes the quantity of oxygene in organized bodies, increaſes at the ſame time their irritability.

We have ſeen a direct proof of this in the 3d experiment, cited above. Beſides this, a great number of other phænomena ſupport my opinion. The irritability of animals made to breathe oxygene air is wonderfully encreaſed. Blanched plants, whoſe irritability has been accumulated in

conſequence

confequence of the abftraction of the ftimulus of light, contain a great quantity of oxygene, according to the experiments of Mr. Fourcroy. I have obferved, in the courfe of my experiments, that plants made to grow in oxygene air become white, although expofed to the light. But what fhows more clearly than all, that the irritability is always in proportion to the quantity of oxygene, are the phænomena attending the action of mercury and mercurial falts upon animals. As this is one of the moft ftriking proofs of my theory, and as I have before obferved, that many perfons, and amongft the reft philofophers of the firft rank, fuch as Dr. Crawford, have been ftruck with the novelty and fimplicity of my mode of explaining thefe phænomena, I cannot forbear entering into fome detail upon this fubject. It is a well-known fact amongft phyficians, that mercury, in its metallic ftate, has no effect upon the human body. I have known many people, who for many years took a daily portion of quickfilver, to the amount of one or two ounces, from an idea of guarding themſelves from epidemic diſeaſes, but who never perceived any effect whatever from this ſingular cuſtom. It is proved alſo by the experiments of Dr. Saunders, that the effects of mercurial ointment are owing only to the ſmall quantity of mercury that has been oxidated during a long trituration. It is neceſſary, therefore, that mercury ſhould be oxidated, to have any effect upon the human body. On the other hand it is well known, that in perſons who have rubbed themſelves with mercurial ointment, or who have taken the oxid of mercury, the mercury, after having produced its uſual effects, has paſſed through the ſkin in a metallic form, and has amalgamated itſelf with watches, and the gold in the pocket, &c. The oxid of mercury, in paſſing through the human body, parts with its oxygene, and it is to this oxygene alone, which remains combined with the ſyſtem, that the effect produced by oxidated mercury is owing. This effect is the mercurial diſeaſe, the ſymptoms of which are the ſame as thoſe of the ſcurvy; the

mouth,

mouth, gums, and the whole ſyſtem are affected in a manner extremely analogous. But the ſcurvy, as I have proved in my firſt eſſay, is a diſeaſe produced by the accumulation of the irritable principle *. The accumulation, therefore, of the oxygene producing the ſame effects, the great analogy between the irritable principle and oxygene appears to be proved, and I think myſelf authorized to conclude, that oxygene is the principle of irritability.

(M. Berthollet, in the Paris Memoires, 1780, has attributed the cauſticity of metallic oxids to the oxygene they contain).

2. Whatever diminiſhes the quantity of oxygene in organized bodies, diminiſhes at the ſame time their irritability.

This has been ſhewn in the 9th experiment, where the heart and the muſcles loſt their irritability, having been deprived of their oxygene by nitrous air. But not to

* Certainly erroneous.

leave

leave any doubt upon this ſubject, I made the following experiment.

Experiment 19. The heart of an animal juſt killed was cut into pieces, and put into a glaſs retort, to which was affixed a pneumatic apparatus. A very ſmall degree of heat was applied to it, by means of a lamp placed under the retort. When the pieces were heated, bubbles of air were perceived in the pneumatic apparatus. They remained expoſed to the ſame degree of heat for nearly two hours, till the ſurface was juſt burnt. Upon examining the air which had paſſed into the apparatus, it was found that the firſt portion of air was the atmoſpheric air of the retort, mixed with a very ſmall quantity of vital air, whoſe preſence was aſcertained by nitrous air. The ſecond was vital air mixed with carbonic acid air.

I have repeated this experiment upon many other parts of animals juſt killed, and I have always obtained a greater or a leſs quantity of oxygene air. It is poſſible to obtain

tain the ſame quantity of this air many times following, by expoſing the animal ſubſtances alternately to the atmoſpheric air, and to a heat of 60 or 70 degrees of Reaumur. I ſhall obſerve, however, that theſe experiments are very difficult to make, and ſome time is requiſite to aſcertain the degree of heat neceſſary to diſengage the oxygene air. If the heat applied be too great, carbonic acid air will come over inſtead of oxygene air. It is poſſible to extract all the oxygene which animal ſubſtances contain, by means of hot water: it is thus we make jellies. Theſe jellies are always more or leſs tranſparent, which, without any other proof, would be ſufficient to authorize us to ſuppoſe the preſence of oxygene in jellies, becauſe it is certain that all tranſparent bodies except alcohol and æther owe their tranſparency to the oxygene that enters into their compoſition*. I have proved, that oxygene combines with venous blood in the lungs; that it is diſtri-

* This is reaſoning very rapidly. Who has detected oxygene in rock cryſtal?

buted

buted to all parts of the ſyſtem by the circulation; that to this principle irritability is owing: it remains only to examine what becomes of the great quantity of oxygene which all parts of the ſyſtem are continually receiving from the blood. I ſhall attempt to prove, that the different ſtimulating ſubſtances abſorb this oxygene.

I have obſerved in my firſt eſſay, that there are three different ſtates of the organized fibre.

1. The ſtate of health or tone of the fibre.

2. The ſtate of accumulation, in which the fibre is ſurcharged with the irritable principle.

3. The ſtate of exhauſtion, in which the fibre fails through want of the irritable principle.

All ſubſtances capable of coming in contact with the irritable fibre, can likewife be ranged under three claffes, of which

The firſt comprehends the ſubſtances which have the ſame degree of affinity to the irritable principle, or the oxygene, as the organized fibre itſelf. Theſe ſubſtances produce no effect upon the fibre.

The ſecond contains thoſe which have a leſs degree of affinity to the oxygene than the fibre has. Theſe ſubſtances, coming in contact with the fibre, will ſurcharge it with oxygene, and produce the ſtate of accumulation. Theſe ſubſtances may be called negative ſtimuli.

The third claſs contains thoſe ſubſtances which have a greater degree of affinity to the oxygene than the fibre itſelf has. Theſe, coming in contact with the fibre, will deprive it of its oxygene, and produce the ſtate of exhauſtion. I ſhall call theſe ſubſtances poſitive ſtimuli.

It is a fact known at this time, that the affinity of different ſubſtances varies conſiderably according to the degree of temperature. The ſame variety takes place in the organized fibre. I ſhall obſerve, therefore, in order to be exact, that when I ſpeak generally of the affinities of the irritable fibre, I mean always in the ordinary temperature of the blood of warm animals.

I will make ſome obſervations upon each of theſe claſſes.

The firſt claſs comprehends, as I have ſaid, ſubſtances having the ſame degree of affinity to the oxygene as the irritable fibre. All organized, or living ſubſtances, are to be ranked under this claſs. (Note, The words organized and living are, in my opinion, ſynonymous. I regard as living, every body, each part of any body, in a word, all organized ſubſtances, as long as they contain the principle of irritability, or of life, and as long as the affinities are the ſame as thoſe of living ſubſtances. The wood, for inſtance, of which our chairs and tables are made, is an organized or living ſubſtance; and to ſpeak properly, it cannot be ſaid that the wood is dead before it be rotten, and ſo of the reſt. Our ideas of life and death are very vague ideas, and I ſhall attempt to fix them in ſome other way). Theſe ſubſtances produce no effect upon the irritable fibre, while their degree of temperature is the ſame as that of the fibre with which they come in contact.

I have arranged, in the third claſs, the poſitive ſtimuli, that is to ſay, thoſe ſubſtances which have a greater degree of affinity to the oxygene than the fibre has. Theſe ſubſtances, coming in contact with the fibre, combine with the oxygene it contains, deprive it of its irritability, and leave it in a ſtate of exhauſtion. There is a great number of theſe ſubſtances. The moſt known are alcohol, ſulfuric æther, opium, and other narcotics, oil of lauro-ceraſus, and oils in general, greaſe, ſugar. All theſe ſubſtances are combuſtible, that is to ſay, they have a great affinity to oxygene, and it is by this property that they deprive the organized fibre of its irritability, by combining with the oxygene it contains. The ſecond claſs comprehends the negative ſtimuli, or ſubſtances, which have a leſs affinity to the oxygene than the fibre has. Some of the moſt terrible poiſons we know of muſt be ranked under this claſs. The oxygene which combines with the organized fibre, when it comes in contact with theſe poiſons, renders it ſo extremely irritable, that the weakeſt ſtimulus is capable of producing death; by a law of irritability which has been explained in the firſt eſſay. Oxygenated marine acid is for this reaſon ſo fatal a poiſon to all organized bodies. It deſtroys them by ſurcharging them with irritability, that is, by hyper-oxygenating them, and becomes marine acid by this operation.

Arſenic, under its metallic form, has no effect upon animals; but the white oxid of this metal is one of the moſt terrible poiſons; for it hyper-oxygenates the organized fibre

fibre with which it comes in contact, and re-affumes its metallic form. Oxygenated metallic falts, fuch as the corrofive or muriatic fublimate of mercury oxygenated, &c. produce the fame effects. The oxids of filver and mercury produce greater or lefs effects upon the organized fibre, in proportion as they contain more or lefs oxygene. The black oxid of mercury, otherwife called æthiops, produces the moft trifling effects; the red oxid of the fame metal produces the moft terrible effects, and deftroys organized bodies in a very fhort time. The fame explanation applies to the action of fulfate of tin and lead, and the acetate of lead and brafs upon the organized fibre.

I am convinced by experiments which I fhall relate fome other time, that the organized fibre, both animal and vegetable, decompofes the water that comes in contact with it. The greateft part of the water we drink is firft decompofed, and then recompofed. It is indeed one of the means by which nature furnifhes organized bodies with

with the oxygene neceſſary to preſerve their irritability and life. This diſcovery explains very many phænomena hitherto inexplicable. I have alſo reaſon to think, that by this diſcovery of the decompoſition of water by organized bodies, we ſhall be able to explain the moſt hidden myſteries of animal phyſiology. Reflecting upon the reſults of ſeveral of my experiments, I begin to ſuppoſe that the hydrogene air, which remains after the oxygene of the water is united to the irritable fibre, may ſerve to ſupply the loſs of nervous fluid, or, in other words, I ſuppoſe that the nervous fluid is the hydrogene air, perhaps carbonic-hydrogene gas. I confeſs this is only a conjecture, which I am not yet able to prove, but which appears to me very probable. Be this however as it may, it is very certain that water is decompoſed and recompoſed continually in organized bodies. This is clear from experiments I ſhall hereafter enumerate.

I have explained the phænomena of hunger in animals; I have ſaid, that this ſenſation

ſenſation was the conſequence of irritability accumulated in the ſyſtem; and that for a ſubſtance to be nutritious, it muſt be a poſitive ſtimulus; namely, one that has a great tendency to unite itſelf to the oxygene, becauſe it is only by uniting itſelf with this principle, with which the ſyſtem is ſurcharged, that it can reſtore the tone of the fibre, and allay the painful ſenſation of hunger. Every phenomenon ſupports this theory. Different ſubſtances nouriſh only in proportion to their affinity to oxygene. Living animal ſubſtances (oyſters for inſtance) afford little or no nouriſhment, becauſe they cannot combine with the oxygene, with which they are already ſaturated; hence the common obſervation, that oyſters increaſe the appetite. Animal jellies, fruits, vegetable ſubſtances in general, afford little or no nouriſhment. Animal food juſt killed does not nouriſh ſo much as that which has been kept ſome time; and raw meat is not ſo nouriſhing as that which has been cooked. Hence all the art of cookery, which conſiſts only in depriving

the

the food of its oxygene, by applying different ſtimulating ſubſtances, and, above all, the ſtimulus of heat. Roaſting the food is the moſt ſimple manner of cooking it; whilſt it is expoſed to the heat, it parts with its oxygene, as in the 19th experiment. Oils, fat, ſugar, alcohol, and other ſubſtances, which have a great affinity to oxygene, are very nouriſhing. In the Eaſt Indies, millions of men ſupport themſelves by ſmall quantities of opium, when the rice harveſt fails them, as very frequently happens in thoſe wretched countries, groaning under the deſpotiſm of a company of Engliſh merchants.

Thirſt is a ſtate of the ſyſtem oppoſite to that of hunger; it is a ſenſation which indicates a ſtate of exhauſtion, a deficiency of oxygene. Every thing that reſtores to the fibre its loſt oxygene, puts an end to this diſagreeable ſenſation. Water produces this effect by its decompoſition when it comes in contact with the fibre. The ſame effect will be produced by vegetable acids, which

which are always decompofed in the ftomach of animals. It is only in proportion to the oxygene in the compofition of the acids, and to which they have but little affinity, that they refrefh and allay the fenfation of thirft. Thus vegetable acids are the beft remedies againft the effects of narcotic poifons; for by their decompofition they reftore to the fibre the oxygene which the poifons had deprived it of. Vinegar, taken in large dofes, cures the ftate of exhauftion produced by a ftrong dofe of opium, and prevents death, which would otherwife enfue. It is well known that drunken perfons become fober by drinking a glafs of vinegar; that is, the vinegar reftores the tone of the fyftem which it had loft, by the effect of the alcohol contained in the wine. A great quantity of water produces the fame effect.

Many other phænomena may be explained upon the fame principle. We find the air frefher and more agreeable after heavy rain, becaufe the watery vapours which riſe from the earth, and come in contact with our bodies, are decompoſed and reſtore the loſt oxygene *. The phænomena diſplayed by the rotifer, that ſingular inſect, which, though entirely dried up, may be revived by moiſtening it with a drop of water, appear inexplicable; but it ſeems that it is eaſy to account for it on my principles. The drop of water is decompoſed, the oxygene it contains combines with the rotifer, reſtores its irritability, its life, and organic motion, of which it had been deprived by the ſtimulus of heat, to which it had been expoſed in becoming dry.

Amongſt the known poſitive ſtimuli, thoſe which produce the greateſt effects are the ſtimulus of putrid fevers, or of the plague, and that of the mephitis, which exhales from putrefied animal ſubſtances in places where the air cannot enter, as in tombs and burial places. The affinity which this mephitic gaz has for oxygene is ſo great,

* Nonſenſe. The air is only become a better conductor of heat.

that

that as ſoon as it comes in contact with the fibre, it deprives it of its oxygene, and cauſes death, frequently in an inſtant. The beſt way to prevent the fatal effects of this gaz is, to detonate nitre upon burning charcoal. The oxygene gaz which eſcapes during the decompoſition of the nitre ſupplies the oxygene which combines with the mephitic air. This theory is ſo true, that the workmen who have been ſuffocated by the mephitic air exhaling from tombs have recovered their ſenſes and been refreſhed (according to their own expreſſion) as ſoon as they have been made to reſpire oxygene air.

I ſhall hereafter relate the experiments I have made upon vegetables, with many ſtimulating ſubſtances, but, above all, with alcohol, opium, the ſolution of white oxid of arſenic, vinegar, water, heat, and the oxids of mercury. I have found that theſe ſubſtances had effects upon plants ſimilar to what they had upon animals; that the irritability of the moſt irritable plants, ſuch as the mimoſa and hediſarum, may be entirely deſtroyed by poſitive ſtimuli, by opium for example, alcohol, or heat; and that it is poſſible to give very ſenſible irritability to plants which did not appear to poſſeſs it, by applying, for ſome time, negative ſtimuli, ſuch as vinegar, or white oxid of arſenic. I hope that the reſult of theſe experiments will be of conſiderable uſe to agriculture, by ſhewing us the true nature of plants, their diſeaſes, and the means of remedying them. I have found that oils and alcohol, employed in ſmall quantities, are ſpecific remedies for the diſeaſes of plants, produced by the accumulation of the irritable principle; diſeaſes marked by the yellow colour which the leaves aſſume.

IT is eaſy to ſupply the latter part of this theory with a neceſſary correction. It is not credible that many poſitive ſtimuli, ſuch, for inſtance, as the vegetable poiſons of the tropical countries, and the venom of certain ſerpents, which produce death, in quantities ſo aſtoniſhingly minute, ſhould, as Dr. Girtanner

ner imagines, exhauſt the irritable principle, by combining with it directly themſelves; neither, ſurely, can alcohol, opium, and oil of lauro-ceraſus, be ſuppoſed to attract from the irritable fibre, if we conſider only the quantity in which they produce their effects, a large quantity of oxygene. Is it not more likely that they occaſion, throughout the whole ſyſtem, a new combination of oxygene? The blood, and the muſcles of animals deſtroyed by poſitive ſtimuli, and of thoſe deſtroyed by negative ſtimuli, as nitre or arſenic, ought, according to this idea, to exhibit appearances diametrically oppoſite.

It is alſo evident, that Dr. Girtanner's, or, more properly ſpeaking, Dr. Brown's, Materia Medica is too ſcanty: negative ſtimuli will be neceſſary to correct certain aberrations of the ſyſtem from health. His idea of negative ſtimuli, as I often obſerved to Brown and his diſciples in 1785 and 6, ſupplies one of the greateſt defects of Brown's ſyſtem. It is in vain to ſay of thoſe ſubſtances which directly, and without any appearance of previous excitement, in whatever quantity they are adminiſtered, ſuch as lead, diminiſh the actions of life, that they are leſs powerful ſtimuli; for how, on this principle, can they leſſen the effect of the ordinary ſtimuli, which are applied at the ſame time? It is equally in vain to ſay (what Dr. Girtanner repeats), that the depreſſing paſſions are only the abſtraction of the ſtimuli of the exciting paſſions. Univerſal experience, I apprehend, will immediately reject ſuch a ſcale of mental affections, as this ſyſtem ſuppoſes; a ſcale which muſt preſent the paſſions ſomewhat in this order; ſuppoſing the moſt exciting to ſtand uppermoſt:

ANGER,
JOY,
HOPE,
CONTENT,
INDIFFERENCE, { As far as the mind is capable of indifference.
FEAR,
SORROW, and ſo on.

Now

Now is it credible or poſſible, that ſorrow ſhould be to indifference what darkneſs is to twilight? Every individual almoſt is capable of judging, whether the thoughts of a perſon in affliction are leſs buſy than of an idiot? whether the mind of one under the influence of the diſtreſſing paſſions is leſs upon a ſtretch, than of him, "Who "Whiſtles as he walks for want of thought?"

During ſuch a ſtate of vacancy, it will hardly, I preſume, be thought that there is more mental exertion than in the moſt profound ſorrow, even if we admit the common ſuppoſition, that in this ſtate the mind reſts upon the contemplation of one or a few ideas; a ſuppoſition which requires much limitation.

Brown very properly warns his readers of the methaphorical nature of his terms.* We

* *Partim ob incertam rei naturam, partim ob ſermonis egeſtatem, item hujus doctrinæ novitatem, incitabilitas modo* abundare, *cum ſtimuli parum admotum, modo* deficere, exhauriri *aut* conſumi, *cum is vehementius incubuit, paſſim*

We must therefore, without regard to them, if obfervation indicates it, admit a power in fome drugs, in fome of the paffions, and

paffim deinceps dicetur. Tum hic, cum alias, ubique REBUS VERIS STANDUM: *lubrica caufarum utpote ferè incomprehenfibilium, quæftio, venenatus ille philofophiæ anguis, cum curâ fugienda. Nequis igitur, per modo relata dicta, incitabilitatis naturam refpici, aut, an materia fit, et fic modo augeatur, modo imminuatur: an adhærens materiæ facultas, nunc vigeat, nunc langueat, definiri: aut ullo modo reconditam quæftionem attingi, quod magno fcientiæ malo femper ferè factum, interpretetur.* l. c. I. 5. 6.

The warning againft the enquiry concerning caufes, as being incomprehenfible, requires fome explanation. Caufes are only phænomena, uniformly preceding other phænomena; they are, therefore, juft as comprehenfible as effects: we ceafe to comprehend juft where we ceafe to perceive. The bane of philofophy is the transferring of phænomena, by the imagination, from one part of nature to another, where the fenfes difcern no traces of their exiftence; and placing them as antecedents before obferved phænomena. As this imaginary fucceffion will be wrong 999 times at leaft in 1000, we fhall as often fail when we attempt, in practice, to modify the confequents, by regulating the fuppofed antecedents.—But, by difcovering the real fucceffion of phænomena, we fhall affuredly gain infinite advantages. Thus, were oxygene the principle of irritability, we fhould not only and in ſome external circumſtances, either of preventing the ſyſtem from giving out its excitability, or from accumulating it (which ſtate is very often ſeen, where a continuance of ſleep, ſufficient as to its duration, does not refreſh, or, according to Cullen's expreſſion, *render the ſyſtem more liable to be affected by ſtimuli of all kinds*), or on the contrary, of accumulating it too rapidly. We are even enabled, by the foregoing hypotheſis, to conceive a modus operandi in all theſe caſes.

Theſe principles, with which the late wonderful diſcoveries of Mr. Galvani, Valli, and Volta, ſeem perfectly well diſpoſed to coaleſce, promiſe all thoſe advantages which would reſult from a perfect knowledge of the mechaniſm of the animal functions. Was

only have the ſatisfaction of comprehending more clearly the actions of the living ſyſtem, but we ſhould have them more in our power, than while we were ignorant of this truth. While, therefore, we religiouſly ABIDE BY FACTS, we can do nothing ſo well as inveſtigate cauſes.

not Mayow, for inftance, infinitely nearer the truth, than any author of a later hypothefis, when he imputed mufcular motion to the *effervefcence of his nitro-atmofpherical particles?* Does not mufcular contraction or intumefcence really depend upon the combination of oxygene with hydrogene and azote (feparately and combined, in various proportions), in confequence of a fort of explofion produced by the nervous electricity? According to this hypothefis, animal motion, at leaft that of animals analogous to man, would be produced by a very beautiful pneumatic machinery; and our nervous and mufcular fyftems may be confidered as a fort of fteam-engine: This hypothefis, though not perhaps at this moment capable of ftrict proof, is extremely probable, fince it is countenanced by every obfervation and experiment yet made on the fubject. It accounts for the perpetual neceffity of inhaling oxygene, and enables us to trace the changes undergone by this fubftance, from the moment it is received, till the moment it is expelled. By the blood it is imparted

to

to the muſcular fibres; here, during their contraction, it combines with the elements above-mentioned into water and various ſalts, among which the marine and phoſphoric acids deſerve particular notice. In this ſtate it is taken up by the abſorbents, and afterwards exhaled, or excreted. Obſervations, adduced in the preceding pages, ſufficiently ſhew the neceſſity of oxygene to muſcular motion; and how the power of motion languiſhes, when this principle is ſcantily ſupplied. It appears that *meat* becomes tender in conſequence of the ſecondary combination of oxygene, in whatever way this ſecondary combination be effected; whether by keeping it till the putrefactive proceſs takes place more or leſs; by cookery; by obliging the animals to undergo violent exerciſe before death, as in hare-hunting, bull-baiting, and in an expedient of gluttony, rather more barbarous than either of the preceding, that of flogging poultry to death. The fleſh of animals ſo deſtroyed ought to be more ſucculent, as well as more tender. It is an obſervation of experienced ſportſmen, that an hunted hare will continue to emit ſteam, when brought to table, very much longer than an hare otherwiſe killed. I have heard the ſame remark made with reſpect to hunted veniſon. Theſe phænomena correſpond perfectly with the ſuppoſition of liquids, partly volatile, being generated during muſcular action.

In the Weſt India Iſlands they kill their poultry with vegetable poiſon, in order to render them tender without keeping. Stimuli, which are only leſs violent poiſons, are ſometimes uſed for the ſame purpoſe in this country. It does not appear whether they produce their effect immediately, or by firſt exciting the nervous electricity. But whatever be the mode of action of theſe poiſons and ſtimuli, that of contagious miaſmata ſeems to be exactly the ſame.* The

* I once ſaw an inſtance, in which I could not doubt that complete intoxication was produced by the contagion of typhus, to which the perſon had been much expoſed. One morning, immediately upon riſing, and I knew

The ſimilarity of the ſymptoms in typhus and ſcurvy has been frequently noticed; and the ſimilar ſymptoms of theſe diſeaſes ſeem evidently to depend upon the ſame cauſe; the contagion of typhus depriving the ſyſtem of oxygene, by cauſing the combination of a great part of that which it already contains. Hence it is probable that the true indication of cure in typhus is to

I knew that he had been perfectly ſober the night before, I was aſtoniſhed to obſerve that flighty vivacity and diſpoſition to wild disjointed talk, together with the other ſigns that infallibly denote a certain degree of intoxication, eſpecially when you are well acquainted before-hand with the manners of the party. In the courſe of the day, during which I ſaw him frequently, he became heavy, had febrile ſhiverings, and complained of head-ache. The next day he became more feveriſh, but was not confined till the fifth day, though the head-ache and other ſymptoms never quitted him. He paſſed through all the ſtages of typhus, but never ſeemed to be in imminent danger. In moſt inſtances, the period of the excitement of the brain is not perceived; we, however, frequently ſee the action of the vaſcular ſyſtem increaſed at the onſet of typhus; this increaſe of action ſometimes miſleads practitioners into the fatal meaſure of blood-letting.—Does not the highly ſaline urine in febrile diſeaſes, and after exerciſe, depend on the chemical combinations above-mentioned?

reſtore

reſtore the oxygene; and it is likely that upon this principle, a certain and ſpeedy cure will be contrived. The modern practice, which employs ſtimulants very freely, though, upon the whole, I believe, not ſo miſchievous as the contrary, is not ſuch as we ought by any means to acquieſce in. It does not enſure ſo much ſucceſs as might be expected from a method founded on juſt principles; and, indeed, as far as I can learn, the different methods in uſe anſwer pretty much alike, and the diſeaſe is very little in the doctor's power—Oxygene may be more beneficial at one ſtage of typhus than at another.

Thoſe caſes where typhus attacks perſons after expoſure to ſevere cold, at a time when you cannot, by the ſtricteſt ſcrutiny, diſcover any previous veſtiges of contagion in the neighbourhood, render it highly probable that this diſeaſe may be produced by ordinary ſtimuli applied to excitability much accumulated. The ſymptoms of the influenza, which are ſcarce diſtinguiſhable from thoſe of catarrh, as well as the effect produced by the ſteward's viſit to the natives

tives of St. Kilda, (if this reſpectably atteſted, though ſurpriſing, narrative be true) afford another inſtance where diſeaſes, extremely ſimilar, at leaſt, are produced by ordinary ſtimuli, and by the extraordinary ſtimulus of contagion. If the marſh-miaſma be not an imaginary being, there is reaſon to preſume the ſame thing of intermittents, which very often appear where marſh-miaſma cannot well be ſuppoſed preſent.

One may, I conceive, reaſonably expect to remove the feveriſhneſs, or indirect debility, that follows intoxication, by cauſing the perſon who is ſuffering under it to reſpire oxygene air. This would, perhaps, not only make up the waſte of this principle, but alſo reſtore the nervous electricity; a circumſtance to which it will always be neceſſary to attend in diſorders of excitement, or produced by excitement. From the experiments of Mr. Sauſſure and Volta, on the electrical phænomena attending condenſation, it may be conjectured that the animal

animal electricity is renewed by respiration. The want of some certain method of effecting this will, perhaps, produce some doubt in the reader's mind respecting the certain efficacy of an hyper-oxygenated atmosphere in typhus; but the few imperfect trials that have hitherto been made upon the respiration of oxygene air, seem to me no more than counterbalance this doubt. The experiments of Mr. Fourcroy, the best upon record, seem to promise the happiest success.

Attention is undoubtedly not less due to the other elements of organized bodies; and if the importance of oxygene seems to have been magnified in the foregoing observations, it is only because we have few or no facts which afford a foundation for reasoning concerning the connection of an excess or deficiency of hydrogene or azote with the functions of life: and yet much obscurity and many difficulties must be expected to remain, till we acquire the knowledge of such facts. This reflection should render us only the more attentive to the phænomena of life; for if we can but perceive enough to ſuggeſt a new hypotheſis, capable of being verified by experiments, phyſiology will not fail to gain ſomething, and perhaps ſomething conſiderable, even by the proof of its falſehood. This reflection ſhould alſo teach us to ſet a due value on our preſent knowledge, though it be imperfect; and it ſhould reſtrain thoſe rude hands that are ever ready to pluck up the tender plants of ſcience, becauſe they do not bear ripe fruit at a ſeaſon when they can only be putting forth their bloſſoms.

A boundleſs region of diſcovery ſeems to be opening before us. Phyſical ſcience, which began with remote objects, now promiſes to unfold to us the more difficult and more intereſting knowledge of ourſelves. This kind of knowledge will aſſuredly, as Dr. Girtanner obſerves, become a part, and the moſt important part, of education; and it will effect a greater improvement in the morals of mankind, than all the ſermons that ever have been, or ever will be preached. Phyſiological ignorance is the moſt abundant ſource of our ſufferings; every perſon accuſtomed to the ſick, muſt have heard them deplore their ignorance of the neceſſary conſequences of thoſe practices, by which their health has been deſtroyed; and when men ſhall be deeply convinced, that the eternal laws of nature have connected pain and decrepitude with one mode of life, and health and vigour with another, they will avoid the former, and adhere to the latter. And as actions are named immoral from their effects, ſelf-love and morality are ſo far perfectly the ſame. Nor is this ſort of morality likely to terminate in itſelf; but the habit of acting with conſideration, and upon principle, will extend from the ſelfiſh to the ſocial actions, and regulate the whole of life.

If it be ſaid, that the general rules of health have long been deduced from univerſal experience, it may be anſwered, that the difference between the perception of two events,

events, which, though ſeemingly allied, are placed at a great diſtance from each other, with a blank between them, and a diſtinct view of the whole ſucceſſion of operations that inſeparably connect them, is immenſe. In the former caſe, there will ſeem to be room for the caprice of chance to intervene and ſeparate them; in the ſecond, there cannot be room for any ſuch deluſive expectation; and this more particular information will render it much more difficult for a perſon to impoſe upon his own underſtanding, than it has hitherto been.

It has always, indeed, appeared to me an eſſential requiſite in a tolerably conſtituted ſeminary of knowledge, to provide the means of popular information on the means of preſerving health; but peculiar advantage may now be expected from ſuch an inſtitution. Its extreme imperfection has hitherto rendered medicine a particular craft, little worth ſtudying, but for the money it would bring. A medley of error, nonſenſe, and contradiction, was not likely to engage volunteer ſtudents; for nothing is more intolerable to moſt minds, than to lie toſſing upon a ſea of doubt. At preſent there is beginning to appear, in phyſiology and pathology, ſomething like the ſimplicity and certainty of truth. In proportion as the laws of animal nature come to be aſcertained, the ſtudy will be gradually eſteemed more worthy of general attention, and in ſpite of the diſguſt raiſed by anatomy at firſt, it will finally prove the moſt popular, as being the moſt curious and intereſting, branch of philoſophy; and a *New Medicine* will at length ariſe, with healing on her wings, from the aſhes of the *Old*

ADDENDA.

ADDENDA.

THE following is the paſſage of the *Flora Lapponica* alluded to at page 58. It were to be wiſhed that the few pages of incidental obſervations, occurring in that work, were printed ſeparately. In ſpite of much quaintneſs of language and puerility of ſentiment, they would intereſt many readers, not likely to meet with them in their preſent ſituation :—

O felix Lappo! qui in ultimo angulo mundi ſic bene lates contentus et innocens. Tu nec times annonæ charitatem, nec Martis prælia, quæ ad oras tuas pervenire nequeunt, ſed florentiſſimas Europæ provincias et urbes, unico momento, ſæpe dejiciunt, delent. Tu dormis hic ſub tuâ pelle ab omnibus curis, contentionibus, rixis liber, ignorans quid ſit invidia. Tu nulla noſti niſi tonantis Jovis fulmina. Tu ducis innocentiſſimos tuos annos ultra centenarium numerum cum facili ſenectute et ſummâ ſanitate. Te latent myriades morborum nobis Europæis communes. Tu vivis in ſylvis, avis inſtar, nec ſementem facis, nec metis, tamen alit te Deus optimus optime. Tua ornamenta ſunt tremula arborum folia, graminoſique luci. Tuus potus aqua cryſtallinæ pelluciditatis, quæ nec cerebrum inſaniâ adficit, nec ſtrumas in Alpibus tuis producit. Cibus tuus eſt vel verno tempore piſcis recens, *vel æſtivo ſerum lactis, vel autumnali tetrao, vel hiemali, caro* recens *rangiferina abſque* ſale et pane, *ſingulâ vice unico conſtans ferculo, edis, dum ſecurus e lecto ſurgis, dumque*

dumque eum petis, nec nosti venena nostra, quæ latent sub dulci melli. Te non obruit scorbutus, nec febris intermittens, nec obesitas, nec podagra, fibroso gaudes corpore et alacri, animoque libero. O sancta innocentia, estne hic tuus thronus inter Faunos in summo septentrione, inque vilissimâ habitâ terrâ? numne sic præfers stragula hæc betulina mollibus serico tectis plumis? Sic etiam credidere veteres, nec male.

HAPPENING, in a company where the Reverend Mr. Leslie was present, to mention my opinion of the possible good effects of air, containing less oxygene than common, in consumption, he related some circumstances relative to the academy at Liege, which he thought gave some countenance to my idea; and afterwards favoured me, at my request, with the following particulars:—

Cum a me, vir eruditissime, postules ut chartæ ea commendem quæ nuper in familiari colloquio asserui circà modum procedendi cum adolescentibus in academiâ Anglorum Leodii in Germaniâ, cum morbo vulgo dicto consumptione vel febri hectica *laborarent, hoc eo lubentius facio, quod exindè ope principiorum artis medicæ forsitan poteris aliqua deducere quæ humano generi plurimum proderunt.*

Notandam imprimis academiam illam, in quâ ego ipse per plures annos habitavi, in vertice alti montis prope arcem civitatis Leodiensis esse sitam, et aerem ibi esse tam purum ut in eo moniales Anglæ, quæ propé academiam

miam olim habitabant, ut plurimùm pulmonum consumptione vitam amiserint, et ob illam causam in infimâ urbis parte, prope Mosam flumen, in denso aere domicilium fixerint, ubi raro consumptione laborant. Exindé partim et ex aliis observationibus mos invaluit in academiâ, adolescentes consumptione laborantes ad loca infima, nebulosa, et paludosa Flandriæ Austriacæ mittere, Brugas puta. Antwerpiam, vel Gandavum, ubi ope densioris aeris intrà paucos menses sanitatem fermé semper recuperabant; si veró ad academiam redibant, iterùm in eundem morbum incidebant. Si cui verò in mentem venerit dubitare an consumptione propriè dictâ laboraverint, ex sequentibus symptomatibus sive indiciis poterit ferre judicium. Laborabant initio morbi tussi fermè continuâ, deinde sputa sanguinolenta mittebant, tum purulenta et fœtida, macilentiores quotidié evadebant, pallidus illis erat ut plurimúm vultus, sed interdum subito roseo colore diffusus, oculi ut plurimum vividi et acuti.

Hæc pauca currente calamo, sine terminorum technicorum ornatû conscripsi; sed si crudus hic scribendi modus displiceat, nihil vetat quominus possis ea, non mutato sensû, in meliorem formam redigere. Vive, vale, vir doctissime, et epistolâ hâc, siquid prodesse possit, rutere.

C. LESLIE.

Oxonii, die 7° Sept. anni 1792.

THE

THE following *conjecture concerning the uſe and effect of manure*, is part of a paper read before the Chemical Society at Edinburgh, April 7, 1786, and printed in that very excellent miſcellany, the Edinburgh Magazine, for one of the two following months. The theory advanced in it ſtill appears to me, ſo far as it goes, to be juſt, and as it is ſo much akin to the ſpeculations contained in the preceding pages, I have ſubjoined it here.—

The reſult of Dr. Ingenhouſz's experiments on vegetables expoſed to the light of the ſun is well known. Since the publication of his Engliſh work, he has been more or leſs conſtantly employed on the ſame ſubject; and on occaſion of ſome controverſies, has publiſhed both in French and German many experiments, all tending to the ſame concluſion. His chief controverſy was with Mr. Senebier of Geneva, which, however, has terminated completely in his favour, for his antagoniſt has publicly acknowledged that he was totally miſled by ſome inattention in conducting his proceſſes. I find too in the Acta Theodoro-Palatina (Vol. V. 1784,) a very long ſeries of experiments by Profeſſor Succow of Mannheim, which exactly coincide with thoſe of Dr. Ingenhouſz.

He concludes his account of them in the following manner. ' Thoſe effects of the ſolar light on plants, which Dr. Ingenhouſz firſt ſo admirably pointed out, are confirmed by the preceding experiments, in which trees and plants appeared moſt capable of yielding pure air in the light of the ſun; whereas in the ſhade they afforded air more or leſs phlogiſticated. That the air which is extricated when

when vegetables are expofed in water to the funfhine, proceeds from their leaves and other parts, fcarce needs any proof. Water indeed does contain a quantity of air which is difengaged by the influence of light; but the quantity is fo fparing even in a large quantity of water, that it can by no means be fet in competition with that which vegetables yield in the courfe of a few hours. Did this air proceed from the water, it would in very few cafes prove fo pure, unlefs the water contained fome of the green conferva; but then it would be to this mofs that the origin of the air muft be afcribed. The difference in the air itfelf which vegetables yield when other circumftances are alike, puts it beyond all doubt, that the air muft proceed, not from the water, but the vegetables.' I fhall leave the fociety to judge how many negative refults will be requifite to deftroy the force of thefe numerous experiments, and proceed to what I have more particularly in view.

Dr. Ingenhoufz, in his laft work, *(Verm. Schr. p.* 394.) has related fome variations of his experiments. He found that water impregnated with acids, alkalis, neutral falts, expreffed juices of vegetables, as of raifins, peaches, &c, very much promoted the production of pure air by vegetables, except in the cafe of the graffes (of which the product is variable from undifcovered caufes), of the conferva rivularis, and water-plants in general, which are killed by fixed air, and fome others, when this acid is employed. The difference is fo ftriking that it will be proper to felect fome inftances.

 $3\frac{1}{4}$ cubic

	Quantity.	Quality.
3¾ cubic inches of a triplex laciniata in common water . . .	3	228
——————in ſtrongly aërated water	9½	286
but ½ was fixed air from the water.		
3 cubic inches of yew, . -	4	206
—————— in aërated water	19	244
of which the pure air was	12	
2 cubic inches of graſs in 86 of common water . . .	5	280
——— in 86 of aërated water .	13	336
N. B. ⅙ was fixed air.		
2 cubic inches of yew in common water . . .	2¾	225
—————— in aërated water	20	322
⅓ fixed air.		

* In 6 hours of pretty fair weather.

	Quantity of Pure Air.	Fixed Air.	Quality.
3 cubic inches of yew in 120 of common water . .	2½	0	234
Ditto with the addition of 2 cubic inches of muſt .	5	a little	270
with 2 of peach juice	5	a little	292
with 2 of ripe apple .	3½	½	250
with 50 drops of aquafortis . . .	4½	⅐	250

* * 6 hours of bright ſun-ſhine.

2 cubic inches of graſs in 86 cubic inches of water .	4	0	248

	Quantity of Pure Air.	Fixed Air.	Quality.
2 cubic inches of grafs with 1 cubic inch of muft .	$9\frac{1}{2}$	a little	312
Ditto of ripe cucumber juice . . .	$2\frac{3}{4}$	a little	230
40 drops of vitriolic acid	$7\frac{1}{2}$	$\frac{1}{6}$	295
a little vitriolated tartar	12	$\frac{1}{4}$	296
* * * fine fun-fhine, 6 hours.			
2 cubic inches of fempervivum tectorium in 86 of water . . .	3	fcarce any	245
Ditto with 1 cubic inch of its own juice . . .	$4\frac{3}{4}$	0	308
* * * * fine weather, 6 hours.			
2 cubic inches of grafs in 86 of water . . .	3	fcarce any	245
Ditto with $\frac{1}{2}$ cubic inch of ftrong vinegar . .	$7\frac{1}{2}$	$\frac{1}{6}$	276
with 1 of juice of the *cornus mafcula* .	$8\frac{1}{4}$	$\frac{1}{8}$	323
1 of juice of onion	$2\frac{3}{4}$	$\frac{1}{5}$	228
1 of juice of turnep	$2\frac{1}{4}$	$\frac{1}{4}$	217

In thefe numerous inftances, the effect of the addition equally appears from the numbers that exprefs the quantity and thofe that exprefs the quality of the air produced. It may be obferved alfo, that a few fubftances, fuch as the juice of onions, cucumbers, and turneps, prevent, inftead of forwarding, the extrication of air, and that it is of an inferior quality to that which is produced when no addition is made.

 In

In the firſt place it is evident, that among the ſubſtances which favour the extrication of pure air, we have every thing which can well be ſuppoſed to enter into the compoſition of manure, ſalts ſimple and compound, with the juices and extractive matter of plants. If we may likewiſe aſſume, that the production of this ſalutary fluid is a natural function and an healthy proceſs, it follows directly, that the uſe of manure is to occaſion a greater exertion of that function.

That the production of dephlogiſticated air is among the chief functions of vegetables is a ſuppoſition countenanced by many experiments. Mr. Cavendiſh himſelf infers, that the vital air obtained by Dr. Ingenhouſz comes from the decompoſition of water. There is one experiment related by the laſt-mentioned author, highly remarkable, and not to be explained on any other ſuppoſition that has been hitherto thrown out. 'I boiled, (ſays he l. c. p. 198. B. 2.) ſome water for two hours, and then poured it boiling into a glaſs balloon of the capacity of 200 cubic inches. The balloon was then carefully cloſed. Before the water was grown quite cold, I introduced into the balloon four cubic inches of granulated green matter, which was taken out of the great reſervoir in the botanic garden (at Vienna), and repeatedly waſhed in boiling water; care being taken to ſqueeze out after each waſhing all the moiſture, in order that none except boiling water might remain adhering to it. I next cloſed the balloon with a perforated ſtopple, in order to allow the water an exit when it ſhould be preſſed by the air evolved from the green matter. The balloon was inverted into a veſſel of quickſilver placed in the

the ſun. The air generated at firſt was abſorbed by the water itſelf: but being ſoon ſaturated, it refuſed to take up any more; and in the courſe of a few days I found a conſiderable quantity of air collected.' If it then be true that water is decompounded by vegetables, it follows that inflammable air is abſorbed and fixed; an opinion countenanced by Prieſtley's experiments of charcoal, on ſliced roots of onions, &c. for neither he nor Senebier, nor Ingenhouſz, nor I think any other, has ever found inflammable air in the elaſtic fluid afforded by the leaves and other proper parts of vegetables expoſed to the ſun. The inflammable matter which ſurrounds certain vegetables is, I ſuppoſe, an eſſential oil in the ſtate of vapour; and if any plant ſhould yield inflammable air in the way I have mentioned, I doubt not but it would furniſh, in other reſpects as well as in this, a ſingular exception to the reſt of the kingdom.

There is ſtill another corollary more preciſe and ſatisfactory to be drawn from theſe premiſes. The quantity of dephlogiſticated air that is extricated, will afford a teſt of the quantity of food taken in by the plant.

It may, moreover, be ſuppoſed, that the additions, which Dr. Ingenhouſz made uſe of, are not thoſe which will produce the greateſt effect. It is reaſonable to ſuppoſe, that Nature in the immenſity of her ſtores, has ſtimulants far exceeding theſe in power; which further inquiry will both diſcover and teach how to apply. For if theſe principles be juſt, they will be eaſily applicable, when we are in poſſeſſion of a greater number of facts, both to gardening and agriculture: And I doubt not but that in time a rational ſyſtem of vegetable medicine may be conſtructed, if the ſubject be properly proſecuted. In the mean time, languiſhing trees may be waſhed or ſprinkled with water acidulated with vitriolic acid, which Ingenhouſz found to be moſt effectual in promoting the production of pure air.

It will not be difficult for any perſon who may chooſe to reflect on the ſubject, to contrive other experiments, by which theſe principles may be confirmed to refuted.

THE END.

POSTSCRIPT.

I KNOW how much MILLMAN infifts upon the craffamentum, obferved by fome authors in fcorbutic blood. But in fuch a cafe I think a very few *negative* more than counterbalance thoufands of *pofitive* teftimonies, where no account is given of circumftances, and no attention has been probably paid to them: the very mode of blood-letting, the time it ftands before it is examined, the temperature in which it is kept, &c. may affect the combination of blood with oxygene, and by confequence, its coagulation.

It is perhaps, in the prefent improved ftate of chemiftry, practicable to afcertain the laws of the coagulation of the blood: M. PARMENTIER and DEYEUX, have not indeed perfectly fucceeded in their refearches on milk, a very fimilar fluid. It is remarkable, that in their experiments the heat of boiling water would not crudle or produce a fkin upon milk without the prefence of air, and yet that it fhould have been indifferent what kind of air was prefent.

N. B. I have lately attended to the colour of phthifical blood; and in fome inftances where it was juft drawn, I have preceived both the *florid* and *claret* or *purple* colour; the former is diftinctly feen, when the blood is fpread thin, the latter, when it has a confiderable depth. This will explain the apparent contradiction in p. 124.